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# ORIGINAL ARTICLES.

## The Geological Map of South Africa.

### By E. J. Dunn.

A SPLENDID map of South Africa published by the Geological Survey of the Union has just reached me. Few will hail it with more pleasure than myself, for I know the country well, having spent fifteen years in helping to unravel its geology. This map marks a great advance on previous geological sketch-maps. To future workers it will be a great boon.

Embodied in it are the labours of scores of workers outside the ranks of the Survey staff, with the result that the Union geology is brought right up to date, an achievement the Survey may well be proud of. Both the general arrangement and the colouring of the map command admiration.

Very clearly it shows that the dominant feature in South African geology is the vast Karroo lake, 800 miles long by up to 350 miles broad, its floor of Dwyka conglomerates and its miles thick of lacustrine sediments. Compared with this the other features are mere details.

When in 1871 my connexion with South African geology began, the geological map of that region was a blank except for Bain's grand work in the southern portion of Cape Colony and Sutherland's in Natal.

Bain's supposed igneous rock of 1856, but which was in reality of glacial origin, formed the southern and western limits of the lake. Sutherland's " Boulder clay " formed a portion of its eastern boundary.

In my 1873 map glacial conglomerate shown on it marked much of its northern limits in Cape Colony.

During 1876–7 several months were spent in making a sketch survey covering about 1,000 square miles of the Stormberg. The several subdivisions were mapped from the volcanics at the summit to the base of the Molteno coal measures. Stormberg Beds were shown extending as far as Middelburg, Transvaal, in the 1875 map.

In 1878 Section 3 of my report on the Camdeboo coal shows the Dwyka coal measures accurately with conglomerate below them and also above them.

## DWYKA COAL MEASURES.

In 1885, after four months travel through Cape Colony, especially in the eastern part right up to Natal, I was able to report to the Cape Government that coal measures much older and thousands of feet deeper in the geological scale than the Molteno coal measures existed beneath the Karroo and directly above the Dwyka conglomerate. On the map accompanying the report the outcrop of the Dwyka coal measures was sketched in through Cape Colony and Natal, which also formed the boundaries of the lacustrine sediments of the great lake and outlined the Lower Karroo or Ecca Beds, Upper Karroo or Beaufort Beds, and the Stormberg Beds, in fact, the whole of the lacustrine series. This was repeated in the 1887 map.

I revisited the Transvaal in 1899, examined the coal seams at Vereeniging, and found Dwyka conglomerate below the coal, glacial pebbles and boulders in the coal itself, and Dwyka conglomerate immediately above the coal.

With respect to the arguments of Mellor and others that the Transvaal coal measures belong to the Beaufort Beds, it proves how hard the fallacy dies started by the late Professor Green, that the Kimberley shales belong to that horizon, for I presume no one will now deny that the Kimberley shales and the Transvaal coal measures are identical in age. No one with extended knowledge of South African geology should entertain such a view, for in the true Beaufort Beds there is no trace of a glacial conglomerate. As to the conglomerate beds associated with the coal seam at Vereeniging being derived by denudation from the conglomerate below the coal, I entirely disagree with the view and maintain that they are undoubtedly as much glacial as the thick mass below the coal. Unless transported by floating ice how could the isolated boulders and pebbles found in the undisturbed coal at Veereniging have got there ? Precisely the same gradual dying out of the glacial conditions as are shown at Vereeniging exist at Blood River, Cape Colony, hundreds of miles away on the other side of the Karroo (see my No. 3 Sec. of 1878).

Under these conditions, the coal and the Dwyka conglomerate being inextricably blended, I abandoned the term Ecca and used the more appropriate term Dwyka coal measures.

On this new map they are re-named Ecca coal measures. With our present knowledge this is a most inept term.

Between the Ecca and Dwyka series a natural boundary is at the top of the coal measures, say the horizon of the remarkable thin bed of cherty rock (white band) that persists all round the Karroo lake basin.

Respecting the Dwyka conglomerate I have always held that it also was laid down in the lake basin. Stratification is clearly marked at Karroo Poort and elsewhere in Cape Colony. That it is penetrated by roots underneath the coal seam at Vereeniging would imply that when the plants grew upon it, it was dry land; but the shales in places below the coal seam, and everywhere covering it, as well as the coal itself, were certainly laid down in water, and so right up to the top of the Cave Sandstone.

Amended boundaries of the outcrop of the Dwyka coal measures completing the circuit through the Transvaal so as to embrace the Middelburg, Natal, and other coals were shown on a map published in the *Transactions* of the Philosophical Society of South Africa in 1900. Bit by bit geological fragments were pieced together until a complete whole resulted so far as the Karroo lake is concerned.

This new map of the Union explains and justifies my advocacy of boring to prospect the Dwyka coal measures in Cape Colony—a matter that has been diligently neglected for forty years.

## LYDENBURG BEDS.

On my expedition through the Transvaal in 1874 I discovered the Lydenburg Beds, so named because they attain their fullest development in that district. From Pilgrim's Rest goldfields I traced them through the Transvaal, recognizing them at Pretoria, Potchefstroom, and on through Griqualand West and across the Orange River into Cape Colony, where I first saw them in 1872 (see *Cape Monthly Magazine* for December, 1872, and January, 1873, and my reports to the Government of Cape Colony). The soft clayey highly ferruginous red shales of Lydenburg village were identified with the intensely hard jaspers seamed with magnetic iron, crocidolite and its beautiful replacements in Griqualand West and the Doornberg of Cape Colony. This formation, thousands of feet thick and of great economic importance, was laid down on my 1875 map, but occupied as I was with other matters and travelling by ox-waggon, and having no maps to work on, the colours could only be limned in roughly.

Five months were spent at the end of 1882 and beginning of 1883 in studying the geology and gold occurrences of this formation from Blyde River on the north to Sabie River on the south. Its eastern limits were defined in more detail in the 1887 map under the title of Lydenburg Beds.

This term is entirely omitted from the new map. Its priority is ignored and the less appropriate term Pretoria Series substituted. Is there such a word as Ibikwa ? The native name for the tamarisk-tree is Abikwa.

## NAMAQUALAND SCHISTS.

In my 1873 map the ancient schists so prominently and abundantly associated with gneisses and granites in Little Namaqualand were named Namaqualand schists. This term was applied in my 1887 map to the similar schists that are gold-bearing in the Eastern Transvaal and Swaziland. The term was appropriate and has a hoary age of over half a century. It is displaced, and Swaziland schists substituted, a far less appropriate term, as very little schist exists in Swaziland compared to the vast areas covered by this formation in Namaqualand. Namaqua schist should be restored.

Other instances could be cited of failure to recognize pioneering work.

Personally I am gratified to find after forty years work by the Survey, aided by so many able European geologists, the correctness of the broad lines of my work so amply confirmed, although carried out 40 to 55 years ago under conditions very unlike those now existing.

# Delta Formation in the English Lakes.

By Thomas Hay, M.A., B.Sc.

ABOUT Easter, 1925, I noticed the curious course of the streams through the big deltas of Measand on Haweswater, and of Aira Beck on Ullswater. Both these streams, after having traversed about half the radial length of the delta, turn fairly sharply towards the head of the lake. This turn in such a pronounced degree is contrary to what one would expect.

The diagrams (Fig. 1) here reproduced by permission of the Controller of H.M. Stationery office from the 6 inch Ordnance Survey Maps will make these peculiarities evident.

One would have expected the mouth of the stream to be either near the medial line of the delta or possibly down lake from that medial position. After some fruitless cogitation on the problem, I thought of a solution which might account for the facts and at once turned to the Ordnance Maps to see if this curious phenomenon was reproduced in other deltas.

It will be noticed that both Ullswater and Haweswater lie roughly north-east and south-west, and therefore the south-westerly gales have a pretty free sweep down both lakes. The south-west is undoubtedly the wind which has the greatest surface effect in these two valleys. Now the streams at Aira Beck delta and at Measand delta both turn to windward and the problem was to account for this fact.

On referring to the Ordnance Maps I found with great interest that there are two other big deltas on Ullswater, both of which show this same curving of the stream to windward, viz., the delta at the mouth of Scalehow Beck and the delta at Sandwick. The curvature is not so pronounced as in the two cases mentioned above, but it is sufficiently noticeable when the curvature is always in this unexpected direction.

Another tracing from the Ordnance Map may now be of interest.

At this point in the problem I wrote to Professor J. E. Marr mentioning these facts about the delta streams and suggesting an explanation. Professor Marr replied that, as far as he knew, nothing had been done on this point. He said that while he agreed as to the

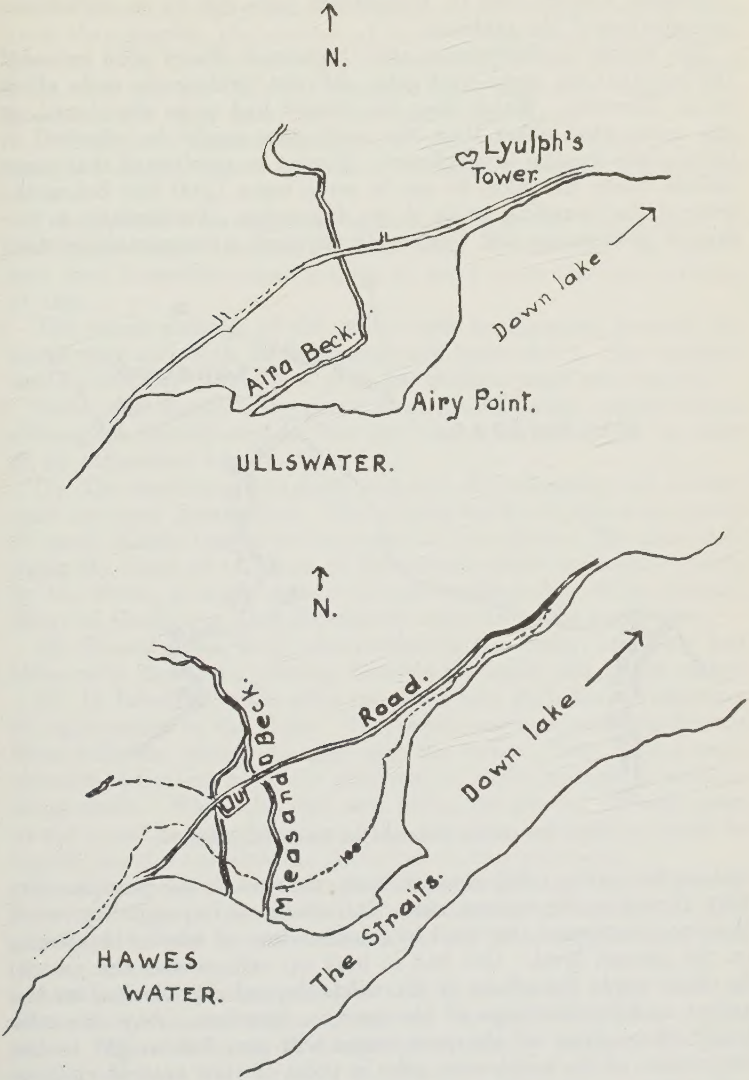

Fig. 1.—Courses of streams in deltas in Ullswater and Haweswater.

general interest of the facts, an examination of the lake shores should be made in order to see if the same result was general.

Before I was able to prosecute the search along the shores of Ullswater, a short visit to Buttermere gave rise to an unforeseen complication of the problem.

The deltas in Buttermere and Crummock Water soon revealed the fact that the south-west gales did not produce the same effect as on Ullswater. Either then the theory had to be abandoned, or else some wind other than the south-west might be effectual in turning the streams to windward. It must be recollected that these valleys where the lakes lie are in some cases 1,500 feet below the level of the bounding walls of the depression. Particularly in the case of Buttermere and Crummock we have a comparatively deep and narrow valley lying between steep walls where the precipices are very abrupt on the western side. It is almost an impossibility to tell the true direction of the wind by consideration of what is happening on the ground level. One has to look up and consult the clouds. In other words the effects of the winds depend a great deal on the aspect and surroundings of the spot in question. Any one who knows these dales all the year round will give full weight to the importance of the south-west gales in point of view both of violence and of the number of days on which the wind lies in that quarter. But these steep-sided valleys provide many places where the south-westers can have little effect and the next most frequent wind may become the one which has most effect in delta formation. The

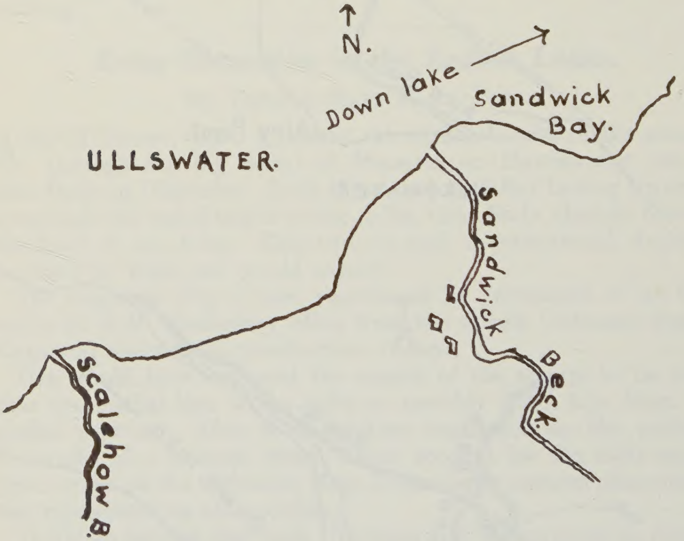

Fig. 2.—Delta of Sandwick Beck, Ullswater.

influence in question depends on strong winds. Consequently it is conceivable that half-a-dozen gales a year might have more influence than all the rest of the winds put together. What is the predominant wind for any special place will depend on the strength of the gales from that quarter, the number of days per year on which gales are blowing from that quarter and the exposure of the place to winds from that quarter, the chief requisite being that there should be a good space of open water to windward of the place.

If there is no considerable space of open water to windward, then the gales cannot raise waves which will be effective in modifying the deltas.

I remembered having read somewhere that the worst gales at Buttermere were the north-westers, and I examined the shores and their immediate surroundings to see if there was any evidence of this.

The whole position of the valley with its direction towards the north-west and with its comparatively open end in that quarter, made this likely on the face of it, but definite proof was wanted.

Such proof seems to be given from the following considerations, although I frankly confess that my time was not enough to allow of an exhaustive examination.

(1) The beaches at the south-east end of Crummock and Buttermere are very pronounced. Big sloping banks of well-worn gravel or small shingle testify to the power of the waves. The pine-trees along the shore at the head of Buttermere show noticeable erosion by the waves, as some of their roots are washed clear of the ground. Many of these trees also tilt slightly away from the north-west.

(2) Several fallen trees were noticed in the valley, and they had fallen with their tops pointing towards the upper end of the valley.

(3) In Lakeland, walls often run down into the lakes in a direction at right angles to the shore. To prevent animals breaking bounds these walls are often prolonged into the water. They form a most valuable indication as to the direction in which material is drifting along shore. When the wall was built, the ground on both sides would be at the same height and the shore line on one side would be exactly continuous with the shore line on the other side.

A wall at the south-east corner of Crummock Water showed 2 yards more of shingle on the north-east side than on the south-west side, and the level was several inches higher on the north-east side than on the south-west side.

Another wall running into the lake in the sharp angle north of Hause Point showed higher shingle on the north side than on the south. The movement of shingle was evidently towards the head of the valley.

For all these reasons I feel sure that the north-west wind is the predominant wind for the Buttermere–Crummock valley. Once again it is important to emphasize this point, viz. that the phrase "predominant wind" here simply means, the wind which has the greatest power of raising waves capable of doing a certain definite piece of work on the shore line.

Thus it seems reasonable to conclude that while the south-west is the predominant wind for Ullswater and Haweswater which run roughly south-west and north-east, there may be other lakes or other parts of lakes where another wind is predominant, and in particular that the north-wester is predominant in most of the Buttermere–Crummock valley.

Rannerdale delta on Crummock Water was the first delta examined, and it brings one up against an interesting point in connexion with this problem. There is a great contrast between the tiny efforts of many delta-forming streams to-day and the huge delta that has been built up in the past. But one must remember that these larger deltas would be built up in many cases at the close of the Ice Age. The streams then descending from the retreating ice would be much more powerful than their small modern representatives. Some of these large deltas may be regarded as extinct deltas to which the modest stream of to-day is adding a subordinate delta of very much smaller proportions. This is especially the case at Rannerdale, for here the stream has actually worked itself clear of the circumference of the old delta and the mouth is situated to the down-valley or windward side of the old delta. Here then, we have a case where the stream has worked in the direction demanded by the theory proposed. There have doubtless been other factors at work in the movement of this delta stream, but to mention these here would only divert attention from the question in hand.

A most curious feature in this delta must be mentioned, which brings it into line with the deltas at Measand and Aira Beck.

If one walks along the delta front from C to B, that is from the leeward corner to the windward corner, keeping not on the shore but along the margin of the field just a few feet from the shore, then one is steadily rising from lake level or little more at C, to a height of about 6 feet at B. That is to say that the windward side of the delta is noticeably higher than the leeward.

The Geological Survey map of Ullswater shows the left hand or south half of the Sandwick delta on Ullswater as a first river terrace, and the right hand half as a later alluvium, clearly showing that the Surveyors recognized a difference between them. There is a difference, but I do not think it is a difference in point of time. Now the left hand half of the Sandwick delta is the windward half on Ullswater where the south-west is the predominant wind. Aira Beck delta shows the same phenomenon, for there the right hand or up-valley half is noticeably higher than the left hand or down-valley half, and this stream empties into Ullswater on the western shore. So that, in the case of the three big deltas at Rannerdale, Sandwick, and Aira Beck we have the windward half very noticeably higher than the leeward. This point will be referred to later on, where its full import will be more apparent.

Just north of Rannerdale delta a huge delta fan has been built by a gully opening directly west. The slope of this fan is so steep that only the lower part, i.e. the part on the lake side of the road, has been brought under cultivation. It shows a well-marked middle runner with very little water, so that it only works on a large scale in wet weather. This stream is building an additional delta where the stream tends down lake and so supports the theory here proposed.

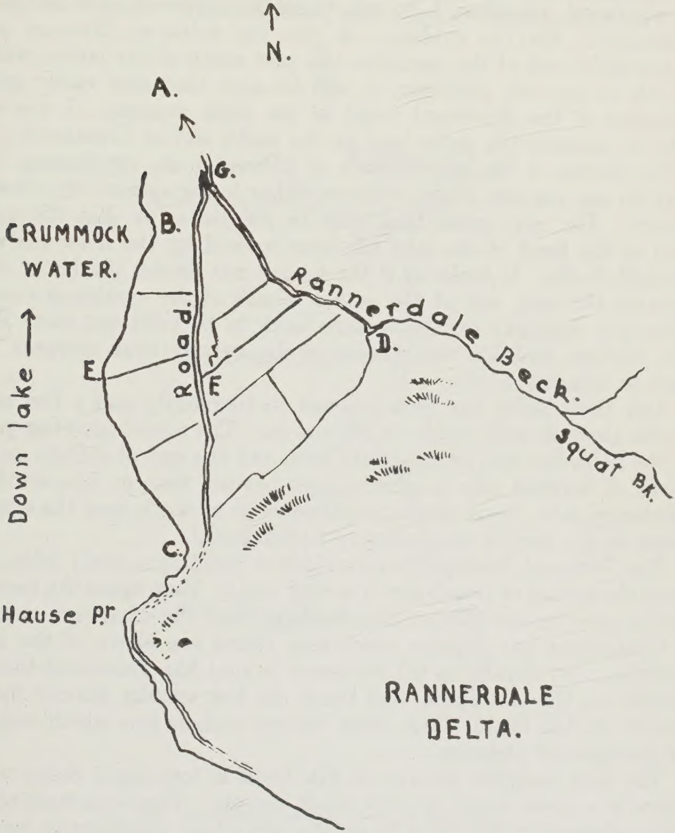

Fig. 3.—Delta of Rannerdale Beck, Crummock Water.

At Hasness near the south-east corner of Buttermere, there are really two deltas separated by a wooded hill. The northerly one has its front now walled up to a height of 2½ feet, and the little stream is kept clear artificially. It need not detain us longer.

Immediately to the south of the wooded hill, Hasness Beck runs nto the lake almost on the down-valley margin of a very perfect delta of semicircular shape. Just as at Rannerdale, it will be observed the stream has worked right to windward and it now opens on the northerly edge of the delta.

Opposite Hasness there is a delta on the west side of the lake at Horse Close, which does not lend any support to the present theory. The stream mouth is fairly near the apex or end of the medial line of the delta but slightly on the up-lake side of that point. But the delta is well sheltered from the west with no space of water to windward, therefore I do not think its evidence need weigh in opposition. On the evidence of the big deltas at Hasness and Rannerdale, and of the nameless one just north of the latter, which are all in exposed positions, it will be seen that this valley gives examples of the windward trend of the delta streams. I was not able to examine the delta land at the north end of Crummock.

The shores of the upper reach of Ullswater are interesting, but they do not contain much evidence either for or against the present theory. The only point that may be mentioned is that the delta land at the head of the lake has been formed by the Grisedale and Goldrill Becks. It looks as if the former has driven the latter over towards the east, but at the actual mouth of the combined stream there is a tendency for the eastern bank to be built out more than the western and for the stream to be curved over towards the west or windward side.

Aira Beck Delta has been referred to frequently and a few more words about it will enable us to pass on. The actual growing point of the delta lies well on the south side, and the spit of shingle on the north or leeward side is growing out beyond that on the south or windward side. Here the same influence is at work, and the stream tends to get turned continuously to windward.

The Townend Beck at Watermillock is building a small delta, but from the nature of the stream it is very small. Here again the leeward spit is growing out further into the lake than the windward one.

Continuing our journey clock-wise round the shore of the lake, nothing need detain us till we leave behind the Basement Conglomerate on the east shore, and reach the low marshy ground by the mouth of Aik Beck. Here there follows a shore line which was full of unexpected pleasure.

The tiny sluggish stream of Aik Beck is forming a delta where there is a good beach of very small shingle. Three-quarters of the circumference of the delta lie on the down-lake, northern or leeward side, and one-quarter on the up-lake, southern or windward side. In other words, in this tiny delta, fully exposed to the south-west, and where a small stream is at work without interference by man, we see the same result being shown, viz. the stream curving round and delivering towards the up-lake or windward side.

Then comes a tiny bay and at once another regular semicircular delta with a small damp but waterless runner turning well up-lake, and again showing three-quarters on the right or down-lake side, and one-quarter on the left or up-lake side. The runner is so small that it is not even marked on the 6 inch Ordnance Map, although the delta is. A small bay comes next, followed by a bigger delta-like growth with one little dip that may have been a past outlet and will deliver in wet weather, and a long artificial straight runner delivering on the south side at a point which roughly gives three-quarters of the margin to the down lake side, and a quarter to the up-lake.

These three little deltas are so interesting that I have traced them from the 6 inch Ordnance Map, and they are shown here. It will be observed that no streams are shown except Aik Beck, but the runners in the case of the other two can be found if they are looked for. The deviation to windward had been first noticed in the great deltas at Aira Beck and Measand, found to be true also in the smaller deltas at Sandwick and Scalehow Force, and again found exemplified in these tiny deltas on the marshy shore to the south of Pooley. I took no accurate measurements and it may be that the segments of the margin in these three deltas are nearer 1 to 2 than

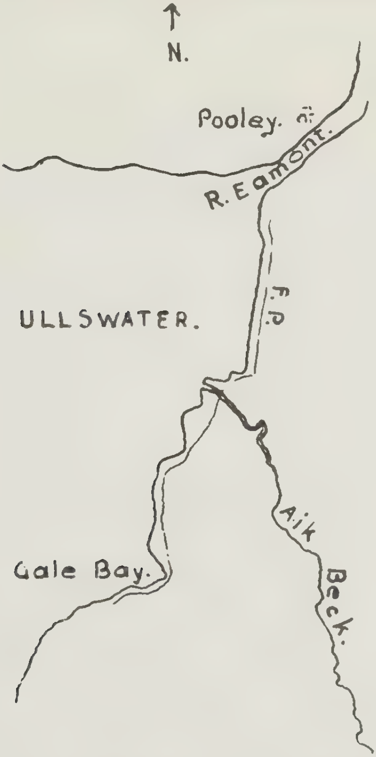

FIG. 4.—Delta of Aik Beck, Ullswater.

1 to 3. But the result was astonishingly regular. Other evidence could be given for Swarth Beck, Purse Bay, and elsewhere. I think enough evidence has been collected to show that in the case of Ullswater and the two lakes at Buttermere there is often a curving of the delta stream to windward. The question then arises how does this result come about? My suggestion is as follows: All the four big deltas that are mentioned at the beginning of this paper lie in such a position that the full blast of the south-west gales is felt after traversing a considerable expanse of water. In this position these gales raise waves of some power which would overcome the downward drive of the delta stream where it meets the water of the lake. It would often happen that the stream would be in spate at the time of a south-west gale, and the stream would then be bringing down detritus in the shape of boulders, stones, earth-wash, and vegetation. The waves would drive this material over towards the leeward side of the stream, in these cases the north-east bank, and in time this bank would grow so much in comparison with the opposite bank that it would tend to drive the stream over towards the south-west.

In other words, I believe the whole cause of the curving to result from shore influence that takes effect within the stream. The symmetrical outline of these deltas has to be accounted for, and one must inquire how such a sideways influence would affect the margin. In any delta when the main stream raises its bed too high or when its course gets blocked or when the flood is so great that the stream cannot contain it, then breaks-away right and left will occur.

If a great flood is on, this breaking out of the water will spread detritus broadcast over the delta, not necessarily over every part equally. The flood in the Glenridding delta of Xmas, 1924, thus spread a huge mass of boulders over the delta and did not result in building a fresh lobe at the mouth of the stream. If then the delta grows mainly in this way by overflow flood action, there is nothing in the present theory to prevent the symmetrical development of the delta margin. Any point in the curving course of the delta stream may at any time become the centre of indiscriminate discharge of detritus. But when the water has established its new course, if the mouth is open to the unimpeded action of the prevalent wind, the same curving influence will once more come into play at the mouth and at the mouth only.

Dr. Marr has with characteristic generosity given me the benefit of his careful consideration of this problem, and he has added to my indebtedness by giving me permission to make use of a valuable suggestion.

He calls attention to the fact that there will undoubtedly be a drift of material along the margin of the delta on the leeside of the stream mouth.

This, combined with the wave action on that margin, will tend to build up a beach which will be influential in closing any distributaries that might form on the leeside in flood time. Another result of this will be a tendency to low-lying marshy land on the leeside of the delta.

I was much interested in this particular point after the wet weather of Xmas, 1925, and in nearly every delta that lay well exposed to the predominant wind, the leeward side was more subject to the formation of pools than the windward. This latter observation is confined to Ullswater, as I have had no opportunity of looking into that point in other cases since my attention was called to it.

May the following sketchy outline now be hazarded as a suggestion of what has happened in these Lakeland Deltas exposed to a predominant wind ?

(1) The delta has mainly grown in extent and depth by overflow flood action directed from some point generally in the medial line.

(2) The predominant wind by driving over movable material at the stream mouth to the leeside of the stream has banked up the leeside and thus driven the stream to windward.

The action, of course, only takes place at the mouth, where the waves act.

(3) The drift along the delta margin to leeward and the wave action in building a beach sometimes higher than the ground behind, account for the low and marshy state of the leeward half in some deltas.

(4) The presence of the stream generally in the windward half of the delta accounts for the greater height of this windward half where this feature occurs.

I have not the least doubt that further study and a more extended examination of Lakeland deltas may lead to modifications or extensions of these ideas. They are only put forward as a first approximation.

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## Radioactivity and the Floor of the Oceans.

By Gerald R. MacCarthy, M.A., University of North Carolina.

RECENTLY a mechanism allowing the escape of the heat generated by radioactivity in the basaltic under-crust of the earth has been proposed by Dr. Holmes,[1] and a very similar one by Dr. Joly [2] has gained wide attention. In these theories the excess heat generated by radioactive changes is supposed to be dissipated by an alternate fusion and solidification of the basaltic magmas assumed to make up the lower levels of the outer crust, the heat escaping through the floor of the oceans during so long a period of time that while, in the aggregate, the amount of heat thus passed out is very great, yet at no time is the temperature of the oceans sensibly affected.

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[1] Geol. Mag., 1925, pp. 504–15 and 529–44.

[2] *Radioactivity and the Surface of the Earth*, Oxford, 1924.

In brief, the accumulation of heat is supposed to become so great that a layer of basalt of considerable, but indefinite, thickness is fused, while the continents because of their low density float in this magma like icebergs in the polar sea. Since the granitic rocks of the continents possess a greater content of radioactive materials than does the basaltic magma, their basal portions are maintained at an elevated temperature, and the basalt immediately beneath them becomes hotter than that which lies beneath the oceans. Tidal forces are now called into play, the continents drift to the westward, and these highly heated areas of basalt come to lie beneath the oceans. The solid basaltic floors of the oceans are then so thinned by upward movement of this heat that the excess heat beneath them can pass through them into the oceanic waters faster than new heat is generated, the temperature of the sub-oceanic magmas falls to the freezing-point, the whole crust again becomes solid, and a new cycle is inaugurated.

Each of these cycles of fusion and resolidification is correlated with a period of diastrophism, Joly considering that each grand revolution corresponds to a period of the basaltic cycle, while Holmes ascribes the grander diastrophic periods to those cycles which are participated in by a deeper-lying peridotite stratum whose cycles come at longer intervals.

These theories suppose that toward the end of a period of heat accumulation and fusion the outermost layers of the crust remain solid and unbroken except for the tensional fractures which may be produced by expansion of the fusing basalt. The continents, as has been noted, float in a layer of fused basalt but, since this latter material expands 10 per cent upon fusion (Holmes), the still solid floors of the ocean cannot be supported by any such flotation effect, but must be supported by the strength which they gain from their dome-like character, or in some other manner. If we assume with Holmes that the density of basalt is approximately 3, and that it expands 10 per cent upon fusion, the density of liquid basalt would be in the neighbourhood of 2·73, so that, by Archimedes' principle, only 273/300 or 91 per cent of the total weight of the solid basalt forming the floor of the oceans could be supported by flotation in the underlying magmas. Should we assume that under the great pressures prevailing at such depths the expansion of fusing basalt is only 5 per cent, we find that the liquid basalt would have a density of about 2·86 and that only 286/300 or 95·3 per cent of the total weight of the floor of the oceans could be supported by flotation. In the former case 9 per cent of the weight must be supported in some unspecified manner, and in the latter case 4·7 per cent.

Hoskins [1] has computed that a dome corresponding perfectly to the sphericity of the earth and formed of a firm crystalline rock of the crushing strength of 25,000 lb. to the square inch and having a weight of 180 lb. to the cu. ft. would, if unsupported from below, sustain only 1/525 (or 0·19 per cent) of its own weight. This result is, according to the computer, essentially independent of the extent of the dome, provided it be continental, and of its thickness, provided the latter be a small proportion of the radius of the earth. The substitution of basalt (crushing strength 30,000 lb. per sq. in. and weight 190 lb. per cu. ft.) in Hoskins' computations would not affect his results to any significant extent. Since the ocean basins are not circular in outline, as were Hoskins' theoretical domes, their strength, considered as domes, would be even less than the above.

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[1] Chamberlin and Salisbury, *Geology*, vol. i, pp. 581–2.

We have seen that, supposing a 10 per cent increase in volume of the fused stratum, only 91 per cent of the overlying material could be supported by flotation, or with a 5 per cent expansion, 95·3 per cent. Adding the support furnished by the dome-like shape of the ocean floors we find that the total support could not amount to more than 91·2 per cent in the one case, and 95·5 per cent in the other. Or, stating the same fact in other words, in the former case 8·8 per cent of the weight of the ocean floor must yet find its support in another manner, while in the latter case this figure becomes 4·5 per cent. If this support be not furnished, they needs must collapse under gravitational stress. The only force to which such a rôle might be assigned would seem to be the hydrostatic pressure of the underlying magmas, which would, *so long as the outer layers of the crust remained unfractured*, probably be amply sufficient for the required support. But so soon as any fracturing occurred (due either to continental drift or to tensional forces engendered by the expansion of the fusing substratum), the liquid magma would pour out under tremendous hydrostatic pressure, and the support furnished by such pressure to the floor of the oceans would be rapidly reduced almost to the vanishing point, and collapse would begin. Fracture of either the continental masses or of the ocean floor itself would, in the end, produce the same result, for even an unbroken ocean floor would founder as soon as its support was withdrawn, although such foundering would probably be more rapid if the fracturing took place in the ocean basins themselves.

It would seem logical to assume (since the tensional strength of acid rocks is of the same order of magnitude as that of basic material) that this fracturing would take place either under the oceans where the outer crust of the earth is most depressed and hydrostatic stresses would reach their greatest magnitude, or along the margins of the continents, for these margins would constitute a zone of weakness. Not only are they the regions above all others where the acid and basic portions of the outer crust adjoin, but a lateral escape of superheated magma from beneath the continents would tend to thin the basaltic crust by fusion. On the other hand, we have evidence from the Columbia Plateau and Deccan basalt floods that fractures reaching the basaltic zone may form within the continental margins. However, as has been pointed out, any fracturing which would permit of a loss of hydrostatic pressure in the suboceanic magmas would be sufficient to initiate collapse of the ocean floors, no matter where the original fissures formed.

Foundering once begun, the solid portions of the ocean bottom would continue to sink into the still molten substratum, while fresh supplies of liquid basalt from below would be forced upward by displacement only to solidify immediately they came in contact with the water of the oceans, and to sink in their turn into the depths below. These solidified rocks of basalt would sink until they reached a solid layer upon which they could come to rest, or until they were re-fused by the heated material below, or until they reached a level of sufficient density to be supported at that level by flotation. Thus, as in the hypotheses advanced by Joly and Holmes, the fused zone of basalt would solidify from the bottom up.

The thickness of the layer subject to fusion has not been stated definitely, although Holmes[1] suggests that it is comparatively thin, while Joly[2] seems to imply that it may reach a thickness exceeding 70 kms. We have seen that, owing to the foundering of the ocean floors, all of this fused material (except, of course, that entrapped in the interstices of the sunken fragments) would be brought in contact with the waters of the oceans before the fused layer could completely solidify once more. If we assume an arbitrary thickness for the molten layer we can very easily compute the heat thus given up to the waters occupying the ocean basins. Assuming the earth to be a perfect sphere of its present cubic content, a zone of basalt 10 kms. thick and whose upper surface lay 20 kms. below the surface of the geoid would possess a volume of approximately 5,049,780,000 cu. kms.; a 20 km. zone would have a volume of 10,093,991,500 cu. kms.; a 30 km. zone 15,128,333,000 cu. kms.; etc. Further, assuming with Holmes that the latent heat of fusion of basalt is 270 cal./cc., it may be readily shown that the 10 km. zone would lose about $136{,}344{,}060 \times 10^{19}$ cals.; the 20 km. zone $273{,}193{,}697 \times 10^{19}$ cals.; the 30 km. zone $408{,}275{,}964 \times 10^{19}$ cals.; etc. If we accept Murray's estimate of 323,722,150 cu. miles ($134{,}166{\cdot}6 \times 10^{19}$ cc.) for the volume of the present oceans,[3] it appears that the heat liberated during the solidification of the 10 km. zone of basalt would amount to 1,017 cal./cc. of ocean; that from the 20 km. zone to 2,032 cals./cc. of ocean; etc. These results are given below in tabular form,

| | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| A | 1,017 | 2,032 | 3,040 | 4,052 | 5,055 | 6,052 | 7,042 | 8,024 | 8,997 |
| B | 10 | 20 | 30 | 40 | 50 | 60 | 70 | 80 | 90 |

"A" being the calories liberated per cc. of ocean, and "B" giving the thickness of the corresponding layer of basalt in kilometres. The first five values are obtained by computation, the last four by extrapolation. In round numbers, each kilometre in thickness

[1] Op. cit., p. 535.

[2] Op. cit., p. 22.

[3] *Scot. Geogr. Mag.*, vol. lv, p. 39.

of a world-wide zone of fused basalt would give up approximately 100 cals. per cc. of oceanic water.

Even this vast amount of heat would not, if its escape into the oceans was spread over a period measured in thousands or millions of years as postulated by Joly and Holmes, affect the temperature of the oceans appreciably. But if, on the contrary, continuously fresh areas of molten rock are exposed on the sea bottom until the entire fused zone has become solid once more, this heat would be supplied at an exceedingly rapid rate over a very short period of time, and the oceanic temperatures could not but be considerably elevated. Even if we assume that 90 per cent of the heat supplied to the oceans escapes during the process, and only the remaining 10 per cent is retained, yet a glance at the table given above will show that the heat given off by a 10 km. zone of magma would be more than enough to raise the present oceans to the boiling point. Any such rise in temperature would, of course, involve a complete destruction of all marine life.

The only escape from this conclusion that offers itself involves the assumption that, as soon as a liquid layer of any appreciable thickness is formed, fissures are produced in the floor of the ocean, and the relatively small accumulation of heat is at once dissipated. But such an assumption also would mean that the discharge of heat no longer occurred in long period cycles, but practically continuously, and the basalt and peridotite cycles invoked by Joly and Holmes as an explanation of periodic diastrophism would be non-existent.

We have seen that any theory involving the periodic dissipation of the earth's internal heat through the alternate fusion and solidification of a basaltic substratum involves a foundering of the ocean floors at the climax of each period of fusion. A necessary corollary is that the present ocean floors, together with their deposits and the oceanic islands in so far as they are not composed of material of low density and hence capable of support by flotation, can be no older than the close of the last basaltic cycle. The periodicity of these cycles is estimated by Joly as in the neighbourhood of 30 to 40 million years, so that at a maximum estimate the present ocean bottoms could be no older than this. Furthermore, we have seen that the escape of heat from even a 10 km. zone of fused basalt would almost certainly be sufficient to raise the temperature of the present oceans to the boiling point. Since neither of these conclusions is supported by the known facts of geology, and since the second is absolutely controverted by palaeontological evidence, it is evident that the theories proposed by Joly and Holmes do not fit the facts, and that some other mechanism of heat dissipation must be sought for. Any modification of these theories which would avoid the difficulties noted above involves the assumption that the escape of the heat generated by radioactive changes is so nearly continuous that other causes for the periodic development of diastrophism must exist.

# Contributions to the Theory of Magmatic Cycles.

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## 1. GENERAL CONSIDERATIONS.

IT is quite clear that Professor Joly's presentation of his theory of basaltic cycles is, taken alone, far too simple to match the complex details of geological history. In a former paper[1] I attempted to extend the principles he has so persuasively advocated, by adding the conception of a peridotite cycle of longer period, and I was careful to point out that even this extension led to no more than a rough approximation to the actual realities of the earth's behaviour. As soon as details are considered, the pursuit of the subject leads its explorer into a maze of difficulties, because of the many interfering factors that must also be taken into account. Nevertheless, a broad survey of the earth's history suggests that Joly has surmised what is so far the only kind of process that even begins to correspond with the dominant facts. And that, without any qualification, is a very great achievement.

Either the earth is in a state of continuous thermal equilibrium or it is not. If it is, then it must be cooling with extreme slowness as the radioactive elements in the rocks gradually disintegrate. In that case the earth would be so nearly dormant that it could have had practically no geological history at all. Evidently then, the earth is not in such a state of thermal equilibrium. It must be (*a*) continuously heating up; (*b*) continuously cooling down; or (*c*) alternately accumulating and discharging heat, this being a discontinuous or cyclic state of thermal equilibrium.

That we are here at all to investigate the problem is sufficient proof that the earth is not continuously growing hotter. The theory of continuous cooling has long been a traditional doctrine of geology, but it has nevertheless consistently failed to lead to any adequate explanations of fissure eruptions and other volcanic and tension phenomena; mountain-building processes and their distribution in time and space; and the alternation of marine transgressions and recessions. The thermal conditions arising out of the Planetesimal hypothesis need not be discussed, as the hypothesis itself has never been generally acceptable, and deductions from it involve quite artificial difficulties of their own. Thus there remains for consideration only the conception of the alternating accumulation and discharge of latent heat proposed by Joly, a conception based on the radioactivity of the rocks and the principles of isostasy and tidal drift. Periodically repeated cycles of fusion and consolidation provide fully for a varied geological history of just the rhythmic and discontinuous type that the earth has actually experienced. No other hypothesis has accomplished so general a synthesis, and at the moment no alternative seems to be even conceivable.

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[1] GEOL. MAG., Vol. LXII, 1925, p. 540.

There is, moreover, direct geological evidence suggesting the alternation of fusion and consolidation in the depths. The great eruptions of plateau-basalts in various parts of the world point beyond dispute to the former prevalence of fusion beneath extensive areas where now the transmission of distortional waves indicates rigid conditions. Similarly, the enormous overthrusts revealed in the Alps and other mountain systems and particularly the tectonic interpretation of these structures which we owe to the genius of Emile Argand,[1] demand a mobile substratum on a regional scale to permit the necessary freedom of movement. Yet to-day, except locally, that substratum is solid. Corresponding to this reversal of conditions in time, the Alpine-Himalayan ranges throughout their length show compressional movements towards and across a medial axis, followed later by tension and stretching, and the development of such features as the Mediterranean basins and the Indo-Gangetic trough.

Several geologists have drawn attention to the apparently abnormal heaving or oscillation of the present polar regions, and Gregory in particular has discussed the evidence in its bearing on the origin of fjords.[2] The phenomenon can be readily explained if there has been an alternation of expansion and contraction affecting the earth as a whole. During expansion the rate of rotation will decrease, with consequent decrease of the eccentricity. Thus the tendency of the polar regions to rise will be accentuated, while that of the equatorial belt will be in part annulled. Conversely, during contraction the rate of rotation will increase; the ellipticity will increase; and the equator will be elongated by an amount equal to twice the shortening of the meridian. Thus the polar regions will sink more than any other part of the world. The equatorial regions act like a feebly compensated pendulum or watch, whereas in the polar regions the processes causing vertical movements enhance each other by acting in the same direction at any given time.

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[1] *C.R. XIII$^e$ Cong. Geo. Internat.* (1922), 1924, pp. 349 and 351. See also L. W. Collet: *Geogr. Journ.*, April, 1926, p. 301.

[2] *The Nature and Origin of Fjords*, 1913, p. 22 and chap. xxiv. See also F. Nansen: *The Strandflat and Isostasy*, 1922, a work which clearly indicates that the subsidiary heaving due to isostatic adjustment during glacial and interglacial epochs is insufficient to explain the present deep submergence of the fjord countries.

From the rise and fall of the polar regions with the attendant fracturing and submergence, we deduce, then, an alternation of expansion and contraction in complete accordance with the theory of magmatic cycles.

It is in such considerations as these, involving the reconstruction of the titanic experiments carried out in the laboratory of the earth herself, that we are most likely to find the solution of our problems. Working along these lines the properties of matter in enormous volumes—very different from those of rock-specimens—can be deduced. Indeed, had radioactivity not been already discovered, it is likely that geologists would have been driven to postulate the self-heating properties which led to the evolution of magmas. Feeling no doubt that " The earth is rude, silent and incomprehensible—at first ", Argand wrote in the prelude of his luminous work, " Nous ne prétendons pas réduire la tectonique à la physique : c'est l'affaire à l'avenir." With the advent of the theory of magmatic cycles that future is definitely begun, for already tectonics have been physically liberated from the deadening conception of fixity, against which—on grounds very different from those of Wegener—Argand has revealed a world of eloquent testimony.

## 2. Mr. MacCarthy's Criticism.

Through the courtesy of the Editor I have been given an opportunity of referring to the preceding paper by Mr. G. R. MacCarthy, a paper in which an attempt has been made to demonstrate that " the theories proposed by Joly and Holmes do not fit the facts ". Two alternative possibilities are visualized by MacCarthy :

(*a*) That the periodic dissipation of heat involves the foundering of the ocean floors, accompanied by such rapid flooding of the ocean floors with magma that the oceans themselves would be raised to the boiling point ;

(*b*) That no periodic climax is reached at all, but that, as soon as a liquid layer of any appreciable thickness is formed, it escapes through fissures in the ocean floor and so dissipates the relatively small accumulation of heat at once.

The first involves a consequence that has not occurred during geological history, and the second reduces the earth's history to a continuous instead of a cyclic process. Both are therefore wrong in fact, and to maintain the theory of magmatic cycles it must be shown, conformably with that theory, where they are wrong in principle.

In the later sections of this paper I propose to show that neither possibility is physically probable ; in the case of (*b*) because no fissures can exist at the great depths where the magma first comes into existence; and in the case of (*a*) because when the magma reaches a level in the crust at which fissures are possible it finds there a light surface layer which can float in the magma.

The principles developed in section 4 A explain why the magma is unable to escape through fissures as quickly as it is formed. There is, indeed, no possibility of fissures reaching down to the zone of accumulating magma until the latter has ascended to within a few kilometres of the surface. The heated rocks immediately above the expanding substratum must accommodate the tension by flowage, and the zone of flowage as a whole stretches outwards and becomes thinner like the stretched rubber of a slowly inflated balloon. Specially weak belts, riddled with the products of fusion, will stretch in preference to, and more than, others, and will thus control the formation and evolution of geosynclines.[1] The rate of expansion is extremely slow, even compared with the flowage of an ice-sheet, and atomic or molecular mobility should therefore be thoroughly adequate to prevent the zone of flowage from bursting and thus giving the magma access to the surface.

According to Mr. MacCarthy the foundering of a gravitationally unstable ocean floor would lead to excessive heating of the ocean waters. I am inclined to agree with the deduction provided that his premises be granted. It has been argued that even if the whole of the ocean floor fell in there would be no serious consequences, because a new crust of solid rock would immediately form, and the rate of escape of heat from the magma would at once be effectively checked.

I have tried to reduce the argument to quantitative terms. Taking the latent heat of basalt at 270 cals. per cc.; the conductivity at 0·004; the specific heat at 0·2; the density at 3·0: and the temperature of the basaltic magma at 1,200° C.; I found by a trial calculation that the time taken to form a crust 250 cm. thick (about 8 feet) would be a few weeks. A more rigorous treatment was applied to the problem by my colleague, Mr. Lyndon Bastings, with the result that the time worked out at about six weeks. The total heat given out in this time would be more than 100,000 calories over each sq. cm. of the ocean floor. Since this passes into a column of water nearly 400,000 cm. high, it is clear that, if divided through all the water of the ocean basins, the temperature would be raised by 0·25° C. in six weeks.

Now a crust 8 feet thick would surely founder in its turn. Probably a much thinner crust would founder under the conditions assumed. But even admitting an 8 foot crust as barely possible, for the sake of the discussion, it follows that 2,000 such crusts would have to form, one every six weeks, before a 5 km. layer of magma could be solidified. That is, for 230 years the oceans would have to dispose of nearly a million calories per sq. cm. per year. The heat received from the sun is about 200,000 calories per sq. cm. per year, and of this about 80,000 are reflected back, 65,000 are used up in evaporation, and the rest is dissipated in melting ice or is radiated away. To get rid of the extra million would involve a slight increase of radiation and in the rate of melting ice, but most of it would have to promote evaporation, and the resulting rainfall would be catastrophic.

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[1] The co-operation of peridotitic cycles appears to be necessary in order to produce the necessary amplitude of movement implied by the greater geosynclines of the past.

The theory of magmatic cycles is therefore gravely weakened, to put it mildly, if it has to admit foundering of the ocean floor on a large scale. I have, however, tried to show that there is good evidence for a light surface layer through which cooling of a heavy magma can occur in a reasonable time without any possibility of widespread foundering.

If Mr. MacCarthy were right in his premises then the ocean floor should have been most easily overwhelmed at times when the greatest eruptions of plateau-basalts took place; for example, between the Jurassic and the Eocene. However, we find instead the oceanic islands then coming into existence. Hawaii is known to have been above sea-level since the early Cretaceous.[1]

This, as we shall see, is inexplicable unless it involved a vast central volcano erupting mainly basalts, followed by intrusions of syenite or some allied type of lighter rock. At this time magma was present beneath both oceans and continents, and the remarkable abundance of plateau-basalts is probably to be referred to the simultaneous existence of a peridotitic magma.[2] But the ocean floor was not overwhelmed.

In some regions the peridotite magma seems to have reached the continents by the end of the Cretaceous, as the serpentines of Baluchistan and Burma and the diamond pipes of Africa and North America bear witness. Although the kimberlite broke through the crust with irresistible explosions, access to the surface led to no widespread flooding of the lands, proving that the phenomenon was mainly due to gas pressure, and that isostatically the magmatic column was balanced with the solid crust.

Even the plateau-basalts, which we should expect to reach the surface more easily, were not erupted in one gigantic catastrophe, as they would have been on the assumption of a heavy ocean floor and a lighter magma beneath. Instead there is a long series of distinct flows averaging perhaps 40 or 50 feet in individual thickness. The intervals between the flows, though not geologically long, were long enough in many cases to permit shallow weathering, and occasionally the growth of soil and vegetation. Thus eruption was the exceptional event; during by far the greater proportion of the time the magma was unable to reach the surface. This can only mean that the ocean floor was not gravitationally unstable.

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[1] C. Schuchert: *A Text Book of Geology*, pt. ii, 1924, p. 548.

[2] Geol. Mag., Vol. LXII, 1925, p. 540. See also this paper, section 6.

Mr. MacCarthy's criticism of the first presentation of the theory is not altogether unjustified, but the different conditions on which the theory of magmatic cycles is based in this paper turns the criticism aside by denying its premises, and by substituting for those premises others that appear to be geologically and physically far more probable.

## 3. SOME NEW DISCOVERIES AND THEIR SIGNIFICANCE.

Before developing the revised theory, it is necessary to state why a revision has become inevitable. Two quite independent lines of work have recently shown that all former estimates of the thickness of the continents were too high, and all former estimates of the radiothermal energy generated within the rocks too low. As a result the theory is greatly strengthened, and its further development is no longer hampered by the faulty data on which, and curiously enough in spite of which, it was originally founded.

Until a few months ago no one had thought seriously of the possible geological consequences of the radioactivity of potassium. However, as the result of a joint investigation by Dr. R. W. Lawson and myself the discovery was revealed that potassium as an emitter of radio-thermal energy is in the aggregate of nearly the same order of importance as the uranium or the thorium family.[1]

Our preliminary estimates are as follows :—

| Elements (In Equilibrium). | *Energy liberated per gram per year in calories.* | *Grams per gram in average igneous rock.* | *Heat generated in calories per gram per year.* |
|---|---|---|---|
| Uranium . . | $7{,}900 \times 10^{-4}$ | $6 \times 10^{-6}$ | $4{\cdot}74 \times 10^{-6}$ |
| Thorium . . | $2{,}300 \times 10^{-4}$ | $15 \times 10^{-6}$ | $3{\cdot}45 \times 10^{-6}$ |
| Potassium . | $1{\cdot}24 \times 10^{-4}$ | $26{,}000 \times 10^{-6}$ | $3{\cdot}22 \times 10^{-6}$ |

The heat generated within the rocks is now known to be considerably greater than the amounts given in all previous estimates. For average granite the heat annually generated per cc. becomes $40 \times 10^{-6}$ calories instead of $30 \times 10^{-6}$ calories. Through a column of unit cross-section (1 sq. cm.) the heat brought to the surface by conduction averages 60 calories per year, and evidently this could be entirely supplied by a thickness of 15 km. of average granite, or of 20 km. of average igneous rock. But the temperature at the base of the column is no longer of itself within the fusion range of basalt-magma and consequently the heat brought to the surface must include a certain amount over and above that due to the granitic rocks alone.

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[1] *Nature*, 1st May, 1926, p. 620.

The *average* thickness of the granitic or *sial* layer (which may be widely departed from in individual localities) must therefore be less than 20 km.

This conclusion is in total disagreement with estimates of the thickness of the continents previously accepted by Joly and myself. The fault, however, lay with the seismologists, on whom we depended, and the mistake on their side has now, fortunately, been detected. In 1924 an attempt was made by Gutenberg[1] to determine the thickness of the continental crust from the data relating to surface waves of known period. Taking

T as the thickness of the surface layer ;
$c_2 = 3{\cdot}2$ km. as the velocity of distortional waves in the surface layer ;
$c^1_2 = 4{\cdot}4$ km. as the velocity of distortional waves in the underlying layer ; and
$c = 3{\cdot}8$ km. as the velocity of surface waves of period 30 secs. ;
he found, $T = 30$ km.

Stoneley then pointed out[2] that it is not c, the wave velocity, that is given by the earthquake records, but C, the group velocity, and that this, which is a consequence of dispersion not previously recognized, affects the validity of Gutenberg's and other similar estimates of thickness.

Jeffreys next worked out the group velocities for a surface layer, corresponding to that of the continental regions,[3] having a thickness T, a density of 2·7, and with $c_2 = 3{\cdot}1$ km. per sec. ; underlain by material of density 3·4, with $c^1 = 4{\cdot}2$ km. per sec. He found that for waves passing through Eurasia, and of the period for which the group velocity is a minimum, the data adopted[4] gave $T = 15{\cdot}2$ km. I have applied the method to most of the results recorded by Gutenberg and find that for Eurasia T ranges from 10 to 20 km. and for America from 11 to 19 km. The thickness $T = 15$ km. is therefore a good average. *For the sub-oceanic regions the same method reveals a surface layer ranging from* 4 *to* 8 *km.* This is a most important result to which I shall later have occasion to refer.

Now if the continents are 15 km. thick on an average, they can no longer be embedded in a basaltic material having a density of 3, for in that case isostasy would demand that they could not rise above sea-level, as a simple calculation shows. They must now be regarded as underlain by material having a density ranging from 3·4 to 3·5 and since the prevalence of basaltic rocks throughout geological time requires that we cannot lightly give up the " basaltic " layer, we seem obliged to conclude with Fermor[5] that the material exists in the eclogite facies.[1] At some unknown depth peridotite will probably constitute the dominant rock. though on account of the similarity in density between peridotite and eclogite it is improbable that seismic observations will lead to the detection of any surface of discontinuity. Seismic evidence does, however, provide definite proof that below the continents the material has the elastic properties and density of rocks such as peridotite or eclogite under high pressure. Beyond the depth where the velocities of the P ans S waves are characteristic of granite or granodiorite, the velocities jump to very much higher values Thus for the P waves the velocities change suddenly from 5·4 to 7·2 or even 8 km. per sec. Experimental work on the compressibility of rocks shows that the velocity of the P wave in a silicate rock of density 3·4 would be 7·4.[2]

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[1] *Phys. Zeit.*, 1924, p. 377.
[2] *Geophys. Suppl., M.N.R.A.S.*, vol i, No. 6, Dec., 1925, p. 280.
[3] *Geophys. Suppl. M.N.R.A.S.*, vol. i, No. 6, Dec., 1925, p. 282.
[4] See B. Gutenberg : *Der Aufbau der Erde*, 1925, Table 49.
[5] *Rec. Geol. Surv., India.*, vol. xliii, 1913, p. 41. GEOL. MAG., 1914, p. 65.

So far there remains only one possible flaw in the concordance of evidence. The geothermal gradient should be higher than we observe it to be. However, that has always been a source of bewilderment. Gradients often seem remarkably low where there has at no distant geological period been indubitable fusion on a widespread scale beneath the continents. Consider the contrasted evidence of the late Cretaceous diamond pipes in Africa and the low gradient of 1° C. in 55 metres (from 2,800 to 4,900 feet) recorded at Dubbelde Vlei in the Upper Karroo, a region that furthermore was riddled with basaltic magma in Jurassic times. I do not propose to discuss the discrepancy here, but it is too serious to ignore and will ultimately have to be faced and accounted for.[3]

Turning now to the ocean floor the assumption that (except for a thin surface layer) the bulk of the material down to a considerable depth has a density greater than 3·4 leads at once to a reasonable solution of one of the problems offered by the oceanic islands. What enables them to stand so high ? The characteristic rock associations of the islands are basalt with trachyte [4] and phonolites, and gabbro with alkali- and nepheline-syenites. Apart from islands which have a granitic foundation, and which are therefore not of the oceanic type, the only available buoyant material that one is justified in assuming is alkali-syenite and related rocks. Former assumptions made it necessary to postulate an incredibly thick column of such rock in order to balance isostatically an island like Hawaii. We now find that a thickness of less than 20 km. of material of density 2·7 would suffice to maintain the elevation. The fact that the superficial lavas are basaltic offers no difficulty in view of the well-supported gas-fluxing hypothesis of their origin now advocated by Dr. A. L. Day[1] and his colleagues. As elsewhere, basalts would precede the intrusive phase.

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[1] P. Eskola : *Norsk Geol. Tidsk.*, vol. vi, 1920–22, p. 168.

[2] E. D. Williamson and L. H. Adams : *Journ. Franklin Inst.*, vol. cxcv, 1923.

[3] See also Van Orstrand : *Econ. Geology*, 1926, p. 145.

[4] R. A. Daly : *Carnegie Inst. Wash.*, Pub. No. 340, 1924, p. 119. *Proc. Am. Acad. Art Sci.*, vol. lx, 1925, p. 75.

The theory also provides an explanation of the subsidence of certain coral reefs with continued upward growth,[2] and the uplift of others well above sea-level. Islands should rise relatively to sea-level after regional consolidation of the substratum has occurred, but those that retain local magmatic activity should shrink from the position of former uplift as the residual bodies of magma consolidate.

Returning to the ocean floor as a whole, the assumption that the light material which must be present near the surface is of the nature of alkali syenite with probably gabbro below, gives a definite physical meaning to the layer of 4 to 8 km. through which the surface seismic waves pass across the oceanic regions. If the density be 2·7, or even if it ranges between 2·6 and 3, there is no longer any danger of a widespread foundering of the ocean floor at times of regional fusion. When the magma in its ascent reaches the zone of fracture it will encounter only rocks that can float.

## 4. THE MECHANISM OF REGIONAL MAGMATIC ASCENT.

### A. *The Zone of Flowage around a Bathylith.*

It is tacitly assumed by Mr. MacCarthy that the magma, when produced, finds it easy to communicate with the fissures produced in the ocean floor. A similar implication appears in another criticism to the effect that in explaining vulcanism Joly's hypothesis explains too much, since it should apparently lead to volcanoes everywhere. The geological evidence, however, is conclusive that deep-seated intrusion is not an easy process and that surface eruptions are essentially discontinuous.

Granites, in particular, seem generally to have found the crust difficult to penetrate, for otherwise there would be far more widespread flows of rhyolitic lavas than actually occur. In chap. iii of *The Ore Magmas* (1923) Spurr shows very convincingly that the gneissic structure of the older grey granites of Manitoba is essentially due to their slow flowage under the influence of heat and overwhelming pressure. The crumpling of narrow veindykes of pegmatite indicates that the flowage took place mainly in a vertical direction and that it was caused by the lateral pressure exerted by later intrusions now represented by a reddish granite. Evidence of this kind suggests that, even though they remain solid, the heated rocks around an advancing magma can flow by a process of slow crystal reorganization owing to the molecular mobility of their constituents.[3] The physical process appears to be similar to that which enables glaciers and ice-sheets to flow.

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[1] *Journ. Franklin. Inst.*, August, 1925, p. 161.

[2] W. M. Davis: *Proc. Nat. Acad. Sci. Wash.*, Feb., 1926.

[3] An important paper dealing with the exchange of atoms in heated solid bodies was read on 23rd April, 1926, by G. Hevesy at the Royal Danish Acad. of Science and Letters.

A bathylithic magma thus appears to make way for itself, in part, by shouldering aside the enclosing rocks. On the flanks these become laterally less voluminous by flowing out of the way downwards or upwards, as required to satisfy the isostatic balance. The rocks above will necessarily tend to be uplifted, but in them flowage will be essentially parallel to the roof of the expanding magma chamber. Thus a protective zone is engendered that is squeezed between the fractured rocks above it and the magma underlying it, and that is subject neither to fragmentation like the one nor to complete fluidity like the other. Acting like a plastic medium it moulds itself to the constantly changing stresses impressed upon it. In accordance with isostasy it must first thicken and cause the surface rocks to be domed upwards. As the bathylith advances it rises (not the rocks as a whole, but a zone determined by the temperature and stress conditions) with the rising geoisotherms, and the actual material of its lower layers flows away laterally and downwards to make room for part of the upward advance of the magma. Ultimately, as the magma ascends high into the crust, the plastic zone of flowage becomes thin and may vanish altogether, but until this limiting stage is reached, it acts in virtue of its molecular mobility as a flexible barrier preventing access of the magma into the zone of fracture above. As the magma advances it carries above it the conditions of its own imprisonment.

The magma will also ascend, and ascend much more effectively, for other reasons. Two cases arise for consideration according as the rocks through which the magma is intruding represent a eutectic with a unique fusion point, or consist of an aggregate of minerals crystallized during a cooling range of temperature. We shall deal with the latter case first, though it matches condition near the surface rather than in the greater depths.

### B. *Crystal Stoping in Rocks that have not a unique Melting Point.*

The phenomena attending the fusion of a deep-seated rock which has not a unique melting point must depend in part on the peculiar properties of surfaces and in part on the thermal properties of the solid solutions which are the individual minerals. The first formed fluid phase will have a different composition from that of the rock as a whole, and the temperature at which it forms will be lower than that at which the whole of the material becomes fluid. Thus the growing magma gradually develops through a range of rising temperature from the first sign of fluidity to maturity.

Now between the highest level where the rising magma is mature (that is at or above the temperature corresponding to its composition and pressure) and the lowest level where the plastic roof is wholly solid, though capable of flowage, there must be a zone in which the first fusion or differential solution along the surfaces of adjacent crystals produces a film-like network of fluid. Sooner or later the crystals will be liberated and will sink into the main body of the magma below. Convection currents and reactions between solid and liquid will continuously restore the magma to the composition appropriate to its thermal state, and the balance of the solid phase will accumulate at the base of the fluid layer or column.

The process of *crystal stoping* involves heating the superincumbent rocks to within the range of fusion and supplying a certain proportion of the latent heat. In order to rise by means of this process the magma must continue to be preceded by a zone of partial fusion and a zone of flowage, and in consequence it will steadily lose more and more heat and crystallize at an increasingly rapid rate as the surface is slowly approached. That crystal stoping is actually a very slow process is made clear by the following hypothetical, but not improbable, example :—

Suppose a magma with a fusion range from 1,000° C. to 1,100° C. has reached a stage of development at which its upper surface has ascended to 15 km. beneath the surface in rocks of its own general composition. The zone of partial fusion will be thin, because in it absorption of heat takes place rather than rise of temperature. The heat lost to the surface by conduction from the top of the zone of partial fusion will be less to begin with than the heat passing from the magma into the zone of partial fusion. Otherwise the magma would crystallize and stoping would ultimately cease. The difference, representing the excess of heat liberated over that conducted away, is alone available to supply the latent heat absorbed within the zone between 1,100° and 1,000° C. If the magma be 10 km. deep, then the excess heat will be about 12 cals. per year over each sq. cm. of the top, plus a small amount due to convection (if any) set up by the pressure gradient component of temperature, plus a further amount due to the possible existence of hotter materials beneath the magma. We shall suppose that these additional supplies balance the cooling to the surface. We shall further suppose that to promote crystal stoping ten per cent of the total latent heat is necessary. Exactly how much would be required to loosen the residual crystals it is impossible to say in the absence of experimental data. The supposition is merely intended to illustrate the process and give a sense of proportion. Taking the total latent heat in the magma at 240 cals. per cc., it will then be necessary to supply 24 cals. per sq. cm. in order that the magma should advance upwards by 1 cm. That will take (at the stage considered) two years to accomplish, since only 12 cals. per year are available from each sq. cm. of the magmatic surface. At a depth of 15 km., where the conditions assumed are probably of the right order, 200,000 years of crystal stoping will enable the magma to rise through only 1 km.

To begin with the rate will be slow ; it will then rise to a maximum. When cooling to the surface just balances the excess of heat brought up, stoping will still go on, because heat will be lost to the surface only from the 1,000° C. geoisotherm, whereas the magma is at the

1,100° C. geoisotherm, and between the two levels there will be an irregular gradient which will make it possible for the latent heat to be trapped within the material in which that gradient exists. At a later stage cooling will be still more rapid, and crystallization of the high-temperature constituents will take place in the magma while fusion of the low-temperature constituents is still proceeding above. Ultimately this process will bring stoping to an end, and the magma, now at the lower temperature, will crystallize without rising further. Stoping will also be brought to an end if at any stage the crystals forming in the magma are lighter than the magma or if for any other reason they adhere to the roof.

If the process outlined above takes place several times through the same materials, then a time will come when further invasions of magmatic conditions will no longer be able to ascend from the original depths by a process depending on a range of fusion temperatures, for all the lower-temperature constituents will already have been swept upwards. The results of continued differential fusion appear to be illustrated by the successive granites of the Pre-Cambrian shields. In Mozambique [1] each of the three granites is richer in potash felspars and radium than its immediate predecessor. The evidence for Finland has been drawn together—though for another purpose—by Sederholm. The average compositions of the granites belonging to four successive eras have been compiled with the following results [2] :—

GRANITIC ROCKS OF FINLAND.

| Oxides. | A | B | C | D |
|---|---|---|---|---|
| $SiO_2$ . . | 66·34 | 67·12 | 70·87 | 72·57 |
| $Al_2O_3$ . . | 15·63 | 15·66 | 14·50 | 12·62 |
| $Fe_2O_3$ . . | 0·99 | 1·23 | 0·74 | 1·45 |
| FeO . . | 3·55 | 2·65 | 1·83 | 2·41 |
| MgO . . | 1·79 | 1·31 | 0·63 | 0·46 |
| CaO . . | 3·72 | 3·04 | 1·81 | 1·34 |
| $Na_2O$ . . | 3·69 | 3·92 | 3·04 | 2·30 |
| $K_2O$ . . | 3·02 | 3·76 | 4·98 | 6·10 |
| $TiO_2$ . . | 0·54 | 0·45 | 0·33 | 0·25 |
| Area in sq. kms. | 53,304 | 60,166 | 33,575 | 18,923 |

A.—Older Granites (mostly Pre-Bothnian).
B.—Post-Bothnian and Pre-Kalevian Granites.
C.—Post-Kalevian Granites.
D.—Rapakivi Granites.

The oldest rock is clearly a granodiorite, and the regular transition to a potash granite is well shown. It is easy to understand why this should be, if B represents, or is derived from, the differential fusion of A; C of B and A; and D of C, B and A. The complementary crystal differentiate in depth must, on this hypothesis, have approached more and more nearly to diorite.

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[1] A. Holmes: *Q.J.G.S.*, vol. lxxiv, 1918, p. 31.

[2] *Bull. Com. Géol. de Finlande* No. lxx, 1925.

Another very important deduction is that the original radioactive constituents must also have been redistributed vertically, the upper and later rocks having been enriched at the expense of the older and deeper-seated rocks. This can be tested, as Joly has analysed many Finland granites for radium and thorium.[1] His results, to which those for potassium are added, are as follows:—

Radioactive Constituents of Finland Granites.

| Element. | A | B and C | D |
|---|---|---|---|
| Radium . | 2·36 | 4·60 | 6·21 × $10^{-12}$ grs. per gram. |
| Thorium . | 0·87 | 2·67 | 5·85 × $10^{-5}$ grs. per gram. |
| Potassium. | 2·51 | 3·61 | 5·06 × $10^{-2}$ grs. per gram. |

We have here a clue to the reason why there have been no later widespread intrusions of granite, and moreover, to the reason for the great abundance of diorites and andesites in zones of later mountain folding. To avoid any misconception it is desirable to point out that stoping of the kind here advocated will occur on the sides of an advancing bathylith more rapidly than at the top. Lateral intrusion may thus be favoured where the bathylith is elongated along a mountain belt. If the magma reaches a place where the foliation planes of the zone of flowage are inclined from the tip of the magma tongue slowly up towards the surface of the foreland then very rapid lateral injection may become possible. This is probably the reason why so many great intrusions formerly thought to be bathyliths are in reality complex laccoliths. The process suggested would explain the presence of the Newer Granites of Scotland on each side of the Midland Valley, the latter representing one of the regions beneath which bathylithic accumulation of magma first began after the Lower Palaeozoic geosyncline, filled with sediments, became compressed and thickened into the Caledonian mountain belt. Lateral injection on both sides would naturally withdraw material from the central region and a rift valley would develop at the surface. Corresponding to the conception of rapid lateral intrusion are the further facts that in the Ordovician igneous cycle vulcanism broke out first at the borders of the geosyncline and only later became important in the central regions of North Wales and the Lake District; and that in Africa the first volcanoes developed on the plateaux bordering the branches of the Rift Valleys, while the still active examples formed later within the rifts, but not on the faults themselves. The lateral injection of the Great

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[1] J. Joly: *Phil. Mag.*, Nov., 1924, pp. 829–30. Unfortunately Joly's paper does not give details from which the B and C granites could be sorted out.

Whin Sill from a source beneath the East Durham syncline is a superficial example of the same kind of process.

### C. *The Problem of Roof Foundering.*

The method of advancing vertically by deformation of the zone of flowage can occur only so long as the magma has an excess of internal pressure over that natural to its isostatic position in the crust. Only quite near the surface (say within two or three kilometres) is it likely that the zone of flowage will become so thin that fracturing and overhead stoping by the sinking of blocks of the country rock can become the dominant process of continued advance. As the condition for this to happen is that the overhead rocks are relatively cold, the rocks will chill the magma, increase the viscosity, promote more rapid crystallization, and in general go far to prevent any slight danger of a massive break through that may still remain if the roof rocks are more dense than the magma. Apart from residual products and effects due to the concentration of volatile constituents the magma should come to rest with a belt of apophyses representing the final arrested effort at block stoping.

The serious danger of roof foundering if block-stoping is the chief method of bathylithic advance has been recognized by Daly [1] as well as by Barrell. The conception of zones of partial fusion and of flowage, with isostatic equilibrium of the column always maintained, and with block stoping as merely a final possibility, goes far to remove what has hitherto been an ineradicable difficulty. The remaining balance of danger could be adequately overcome by the development of a central volcano above the bathylithic focus, providing thus a safety cap into which the still unexpended magma could continue to force and insinuate itself without flooding the surrounding country.

The above conception of magmatic intrusion on a large scale has obvious bearings on petrogenesis, and others which can be developed, Here, however, I wish to apply the principles involved to the ascent and cooling of the magma formed below the ocean floors.

### D. *Crystal Stoping in Rocks that have a Unique Consolidation Point.*

In striking contrast to the range of composition exhibited by the Pre-Cambrian granites is the fact that throughout the world plateau-basalts are characterized by one nearly uniform type of chemical and mineralogical composition.[2] This points, as we shall see again later, to the fusion of a high-pressure facies of basalt, thought to be eclogite, which has a unique consolidation point, and which is therefore a eutectic aggregate. Magmatic conditions will not be able to advance through such a medium by the process of crystal stoping already described.

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[1] *Igneous Rocks and Their Origin*, 1914, p. 205.

[2] H. S. Washington: *Bull. Geol. Soc. Am.*, vol. xxxiii, 1922, p. 165.

In this case the condition for crystal stoping is that the magma should be able to grow along the crystal boundaries more rapidly than on the under surfaces exposed to the magma by the individual mineral components of the eutectic. The temperature of the magma will also need to be a little above the eutectic point in order to provide the heat necessary to enable mutual solution to proceed. Diffusion from the solutes will be extremely slow, but along the boundaries it will necessarily be more rapid than elsewhere. The first supply of latent heat will produce a film of fluid from the boundaries of say, crystal A and crystal B. Thereafter diffusion of A and B into the fluid will take place, and the products will come into contact and add to the eutectic solution almost as quickly as they are liberated from the crystal network. Away from the boundaries, the meeting of the diffusion products of A and B will be much longer delayed; it will be inappreciable, indeed, unless the temperature of the magma is well above the eutectic point. Thus the roof will be corroded along the junctions of the crystals, and the latter will become so honeycombed with the eutectic solution that the residual crystals will soon become free to sink into the magma below. In its mechanism this process of crystal stoping is similar to that already described, except that the magma itself cannot crystallize at the same time, since the process stops if the temperature falls below the eutectic point, or if cooling to the surface takes place more rapidly than latent heat can be supplied.

## 5. THE GROWTH AND ASCENT OF BASALTIC MAGMA.

If the material here assumed to be eclogite, from the fusion of which basaltic magma originates, has already passed through former states of fusion and consolidation, then the crystal aggregate left at any level must have a unique consolidation temperature corresponding to that level. For suppose, instead, that it consolidated like magmas nearer the surface through a range of temperature: Then at any point the first threads and films of fluid would be rich in the low temperature constituents of the solid solutions, in this case the alkali-felspars. The upper part of the magma would therefore tend towards a syenitic composition, and the residual crystals that sank to the bottom would be impoverished in the low temperature constituents. This result may well have been achieved in the earth's early days when the light surface layer of the ocean floor was differentiated.[1] But it clearly could not happen many times on an important scale, and, after being thoroughly worked through, the material of the substratum would inevitably come to have a unique consolidation point. Corresponding to this deduction is the remarkably uniform composition of the plateau-basalts all over the world.

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[1] For a suggestion with regard to the origin of the granitic rocks of the continents and their original distribution, see A. Holmes: *Nature*, 6th June, 1925, p. 873.

It is suggested that the eclogite of the depths is a high-pressure eutectic, for if it were not, then the earliest lavas of any volcanic cycle would be alkali rocks. Actually, however, the latter and the phenomena of spilitization are practically always and everywhere preceded by normal basalts; they are due to differentiation of the magma during its consolidation in higher levels, not to differential fusion during its growth at lower levels. It is of interest to note that Asklund has shown that the pyroxenes of plateau basalts all cluster along the bottom of the eutectic valley mapped out by the temperature contours in an equilibrium diagram; and further, that the composition of the felspars is such that differentiation of the plateau-basalt magma into granitic material is impossible.[1]

Under volcanic conditions basaltic magma seems to be capable of producing a residual trachytic magma, but this could not happen where eclogite crystallizes from the magma. Between these extreme conditions there is probably an intermediate stage in which high-pressure calcium and ferromagnesian minerals can separate, but not high-pressure alkali minerals, in consequence of which a residual magma from which the alkali rocks can be generated is left.[2]

In considering the growth of basaltic magma, represented in its quenched form by the plateau basalts, we have then to deal with the melting phenomena of eclogite having a unique consolidation point and of sufficient thickness to give rise to magma. The beginnings of fusion will take place from the base of the eclogite layer up to the level where cooling to the surface has just penetrated. The fused zone will grow thicker under the oceans than beneath the continents, because in the preceding cycle of fusion the substratum will have become solid beneath the continents while the thin ocean floor is still carrying off the latent heat of the remaining magma. For this reason the thinner substratum beneath the continents will have cooled to a considerable depth while that beneath the oceans is still all at its fusing point. Below the greatest depth of cooling the ceaselessly generated heat will be accumulating throughout the period of cooling, and evidently more will accumulate under the oceans than under the continents. Thus, almost from the start of magma growth, the ocean floor will begin to be buoyed up and the sea-level to rise, relative to the continents, by the slight excess of expansion taking place beneath the oceans.

The growing magma will gradually be squeezed upwards to form ultimately a continuous layer at the top of the zone in which it is being generated. When the temperature of the top of the magma has risen above the consolidation point of the eclogite immediately above it stoping will commence, and the magma will be fed from above as well as from below, the latter source of supply being by far the greater. The top of the magma will be kept hot for two reasons. The temperature at the bottom will be higher because of the greater pressure, and within the magmatic layer the radioactive elements will continue to generate heat. Thus, whatever the relations may be between compressibility and thermal expansion, convection currents aided by tidal disturbances will sooner or later bring to the top an excess of heat. In addition the new magma being generated below will be at a higher temperature than that normal to the accumulated layer into which it is squeezed, and thus still more excess heat will be available.

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[1] B. Asklund: *Sveriges Geol. Undersökning*, Ser. C, No. 325, pp. 75 et. seq.

[2] J. W. Evans: *Q.J.G.S.*, vol. lxxxi, 1925, p. cx.

The magma will rise by stoping, and the cooler products from the top will sink (both solid and fluid) and to some extent stem the upward accession of heat. At first the supply of heat available for promoting stoping will increase as the volume of magma increases, and this will continue till a maximum volume is reached when the increasing rate of cooling through the roof (which is steadily becoming thinner) just begins to balance and overtake the process. From this time onwards stoping will take place at a decreasing rate. If the magma thickness is small it may die out altogether during this stage. With a greater thickness of magma stoping may continue until cooling to the surface just balances the rate at which latent heat can be supplied from below to the upper layer in contact with the magma. The magma will then remain permanently in equilibrium and will never cool unless intrusion can continue by some other process. Before this, however, the magma may have reached the under surface of the syenitic or granitic (sial) layers. Here it will come to a standstill (unless it rises by some other process), but as the crust is now carrying away heat more rapidly than it is being brought in, the magma will cool until crystallization can begin. It will then be reduced in volume, and provided the crystals sink, the process of cooling will continue until the whole is solid.

The various possibilities are briefly these :—

(*a*) The eclogite zone may be so thin that cooling to the surface keeps it solid throughout.

(*b*) The eclogite zone may fuse from below by heat brought up from the peridotite layer.

If the eclogite zone is thicker than the maximum depth ever reached by cooling, then that part which is either below the maximum depth reached by cooling or below the accumulated magma will steadily supply new magma which migrates upwards, the amount per year depending on the thickness of the zone of supply and, as in (*b*), on the heat from the peridotite layer. The accumulating magma now rises by stoping through the zone of cooling. Three different cases arise :—

(*c*) The magma may die out altogether, by cooling more rapidly than it grows, while still in the eclogite zone ;

(*d*) The magma may reach a level at which equilibrium is established;

(*e*) The magma may reach a surface (*sial* of the continents or ocean floor) at which it stops rising, and proceeds to cool and solidify. In cases (*c*) and (*e*) cooling afterwards penetrates downwards to the maximum possible depth before magma can again begin to accumulate at that level.

The process described by Joly in his presentation of the theory is assumed without further discussion to lead to what is in principle case (*e*). An objection to the theory which, if it were valid, would be fatal, has recently been voiced by Jeffreys.[1] He doubts whether a magma once formed in the manner described by Joly could ever again solidify. That is, he assumes that the theory most probably leads to case (*d*). Certainly, when simple melting of the roof is envisaged without the accelerating aid of stoping, the rise of magma is bound to be very slow, and to that extent case (*d*) is favoured. The conception of stoping, which is physically valid, increases the probability of stages (*c*) or (*e*) being reached; but clearly the particular case that actually corresponds to the reality must depend mainly on the thickness of the eclogite layer and its thermal properties. It also depends to a less extent on the heat being received from the region underlying the eclogite layer. If the material of that layer be peridotite then the difference in fusion points will probably be at least 300° C., and consequently the heat passing upwards into the eclogite zone will be appreciable. This conclusion is, moreover, of the utmost importance in connection with the *modus operandi* of the suggested peridotite cycles and their interaction with the eclogite layer.

We can safely rule out case (*d*) as being impossible in the earth, since the outer layers are now solid. It then remains to decide whether the eclogite zone is sufficiently thick for cases (*c*) or (*e*) to arise. If it is not then the whole theory fails, and we are obliged to return either to the theory of a solid cooling earth which I originally proposed,[2] and which is still advocated by Jeffreys, or to transfer the conception of magmatic cycles to the peridotite zone. Since we have no knowledge whatsoever of the thickness of the eclogite layer from seismic data, geological history becomes the only court of appeal. I have already rejected my former theory of a solid cooling earth for geological reasons which I believe to be conclusive, reasons that have become still more weighty since the discovery of the thermal importance of potassium. Again from the geological evidence I have reached the conclusion that both basaltic and peridotitic cycles are necessary to explain the complex rhythms revealed by the earth's history.[3]

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[1] *The Observatory*, Feb., 1926, p. 56.

[2] GEOL. MAG., 1915, pp. 60 and 102; 1916, p. 265.

[3] GEOL. MAG., 1925, p. 540.

It may therefore be assumed as a good working hypothesis that the eclogite zone which is the home of the basaltic cycles *is* thick enough to lead to the observed results. I am anxious to point out, however, that these results are not yet known to be physically inevitable. This knowledge may come in time, but at present we have not the requisite data to assert that basaltic cycles *must* happen. We can, however, argue from the facts of geology that they have happened, and we can interpret the conditions of their evolution accordingly. The evidence further suggests that the consequences due to basaltic cycles acting alone are feeble compared with those produced by the longer and more powerful cycles referred either to the peridotite layer alone, or to the interaction of its magma with the eclogite zone.

Obviously the time involved in the growth of the maximum thickness of the magma can no longer be evaluated by the simple latent heat method, even if we assume that the latent heat of the change of state from eclogite to magma is not very different from that of basalt. The method depends on the assumption that all the solid material below the deepest level of cooling (or an equal mass) ultimately becomes fused. The process here pictured gives no assurance that this must happen, for it involves crystal stoping and the continuous production of magma from below. Though the thermal problem involved in relation to time could probably be approximately solved by appropriate mathematical treatment, the data are again lacking from which an estimate of the duration of a cycle could be made.

## 6. THE CONSOLIDATION OF THE BASALTIC MAGMA.

Below the continents the magma will rise till it reaches the lighter crust. The rocks there are probably diorite or granodiorite, but the non-committal term *sial* expresses better the range of possibilities. Cooling will then take place, and, as a result of the transfer of heat, the sial will begin to fuse and will thus set up a crystal-stoping process of its own. This will proceed until it is arrested above by the increased cooling to the surface, and below by the solidification of the basaltic magma. Since the close of the Pre-Cambrian the ascent of sial magma has not reached levels, except in favoured situations, where the resulting rocks could later be revealed by denudation. Where the sial is thick, however, as in newly compressed mountain belts, the conditions have been favourable for the development of andesitic volcanoes and the granodiorite type of elongated bathylith.[1]

Below the ocean floor the basaltic magma reaches a level 5 or 6 km. higher in the crust than the base of the average continent. Here again the heat may be taken off by partial fusion of the sial as well as by conduction to the surface. In this case, however, the rate of supply of heat to the waters of the ocean will be too slow to affect sensibly the thermal state of the latter. Probably far greater quantities of basaltic magma will rise through fissures, expanding under the relief of pressure, and to a limited extent flowing over the ocean floor or building central volcanoes. Magma in contact with a syenitic layer undergoing fusion would inevitably become enriched in the constituents of the alkali felspars, and matching the expectation we notice that many of the basalts of oceanic islands differ from plateau basalts in containing higher percentages of soda, potash, alumina and silica.[1]

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[1] For the application to the well-known andesite-rhyolite-basalt cycles of America and Japan, see p. 327.

As the sial of the continents is about three times as thick as that of the oceanic surface layer, cooling takes place through the latter much more rapidly than through the continents. Nevertheless, when the continental base is just grounding on the floor of precipitated crystals, 5 or 6 km. of magma will still exist beneath the oceanic surface layer ; the reason being that the magma is always at the top while the crystals due to cooling accumulate at the bottom. The extent of marine transgressions shows that they could be fully explained by a ten per cent volume change in this part of the material of the substratum.[2] The ensuing solidification of the sub-oceanic magma will thus cause the previous overflow of the ocean waters to be withdrawn from the lands.

It may be urged against this interpretation that the volume change from eclogite to magma may be very much greater than ten per cent. At normal pressure it can be as much as thirty per cent,[3] but the effects due to compressibility must be considered when we are dealing with materials at thousands of atmospheres' pressure and at a temperature that may be well over 1,200° C. The volume change in solids is generally between one and ten per cent of that produced in liquids over the same range of pressure. The density of a rock normally at 2·70 is raised by the application of 4,000 atmospheres to 2·72. The same pressure on a magma of density 2·7 should raise the density to at least 2·9 and possibly very much more. Moreover, the compressibility of most solids, and of all liquids that have been tested over a wide range, rises with temperature. In the case of mercury, for example, the compressibility at 2,500 to 3,000 atmospheres is raised by 20 to 25 per cent when the temperature is raised only 170° C. Unfortunately we have no data for silicate melts, but all the analogous evidence suggests that the density of basaltic magma at the level of the continental base is likely to be not less than 3·1. Certainly it must be far higher than 2·7.

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[1] See for example H. S. Washington : *Am. Journ. Sci.*, Oct., 1923, p. 361 (Hawaiian averages) ; *Bull. Geol. Soc. Am.*, vol. xxxiii, 1922 (Plateau-basalt averages). W. Campbell Smith : *Q.J.G.S.*, vol. lxxxii, 1926, p. 44 (Christmas Is. analyses).

[2] A greater volume change would imply less than 5 or 6 km. of magma beneath the oceans when the sub-continental layer became solid.

[3] L. L. Fermor : *Rec. Geol. Surv. India*, vol. lviii, 1925, p. 220.

Thus in the sub-oceanic region, at the stage when the continents have just grounded or have otherwise made solid contact with the substratum, we seem justified in visualizing a magma say, 5 km. deep; followed by, say, 6 km. of rock much lighter than eclogite, and probably consisting largely of syenite; followed in turn by nearly 4 km. of clean water. If the density of the magma be 3 to 3·1, and that of the surface layer 2·7 to 3·0, then there is no possibility of foundering even if the fractures now extend right through the surface layer. Cooling will continue partly by eruption and partly by crystal stoping in the surface layer and conduction, and in half a million years or less the remaining magma will have completed its consolidation.

## 7. THE EFFECTS DUE TO A PERIDOTITE CYCLE.

We must now consider what is likely to happen in the zone beneath the eclogite layer. The material there is generally supposed to be of the nature of peridotite, and we shall adopt that view as a working hypothesis, and make the further assumption that the radioactivity of the material is low compared with that of eclogite or basaltic magma. Judging from the melting points of pyrope and alkali clino-pyroxenes (not far from 1,200° C.) eclogite is certain to be fluid several hundred degrees below the temperature at which a magnesium-rich peridotite magma could begin to form. The low temperature constituents of peridotite, and the proportion of the radioactive elements associated with them, would be swept upwards by the earliest waves of magma, and therefore need no longer be considered. The material left should be rich in forsterite, and it may therefore be expected to have a fusion point of the order 1,600°–1,700° C.

*Stage I.*—When the conditions in depth are essentially solid the lower part of the eclogite layer will be at its fusion point, and from the base downwards the temperature will continue to rise until the temperature of fusion of the peridotite zone at that depth is reached. At that level and below magma will form with extreme slowness, and only after many millions of years will it be possible for any appreciable amount to accumulate into a definite layer at the top of the zone of fusion. Upward ascent is also likely to be a far more tardy process than in eclogite, since our peridotite is pictured as an essentially monomineralic rock. Little or no range of temperature is available to promote crystal stoping, and for the most part ascent will be possible only by the melting away of the upper rocks. Thus, in the regions above the peridotite zone several eclogite-basalt cycles may run their course while the peridotite magma is slowly growing and working its way up to the base of the eclogite layer.

During this interval of steadily increasing expansion the earth's surface will be necessarily extended, and the effects of the minor basaltic cycles will be merely superimposed. When a basaltic cycle is in its phase of contraction to eclogite the net effect will be to reduce the extension of the surface. Obviously this may or may not cause contraction and folding, according to the proportions of the two magma layers and the way in which the surface has already adapted itself to a greater radius. There is room for a wide range of possibilities here, but only four points of noteworthy importance need be specially referred to.

(1) Marine transgressions and recessions will proceed as before, since they are controlled by the growth and consolidation of basaltic magma and the fact that the sial of the continents is thicker than that of the oceanic floor.

(2) The first basaltic contraction will be but little affected by the growth of the peridotite magma and is therefore likely to be responsible for minor mountain-making movements.

(3) The later basaltic magmas will find the crust above them thrown into tension more and more as the peridotite magma grows, and they should therefore be particularly liable to reach the surface as plateau eruptions. This corresponds with the eruption in different parts of the world of the post-Triassic plateau-basalts.

(4) The growing tension should facilitate the equally gradual development of geosynclines, and their evolution should be of a pulsatory type on account of the superimposition at intervals of minor compression. The history of every reasonably well-known geosyncline corresponds to the deduction.

*Stage II.*—As soon as the peridotite magma reaches the eclogite zone conditions become very different. Stoping will take place more rapidly than at any other time, and the bulk of the eclogite will be rapidly transferred to the bottom of the magma. The magma may thus reach the basaltic magma and begin to cool there, while the basaltic magma is either growing and ascending, or cooling and dying out. The only other possibility is that it reaches the surface layers of sial and there begins to cool, and in any case this is bound to happen sooner or later, since the solid products from any basaltic magma above the peridotite magma will sink to the bottom of the double layer. Here again there is room for a great deal of variation in behaviour and effect. The most important general consequence will be considerable tension followed fairly rapidly by compression on a scale corresponding to the volume of magma that solidifies. To this stage is referred the Laramie orogenesis of the close of the Cretaceous, and the serpentine, kimberlite or peridotite intrusions of Baluchistan, Burma, New Caledonia, New Zealand, Arkansas and other North American localities and South and Central Africa. Elsewhere continued eruptions of plateau basalt are to be expected, and in fact they actually occurred ; in Western America and Japan the first of the andesite-rhyolite-basalt sequences took place in the Eocene.

*Stage III.*—Closely following on Stage II, and overlapping it locally with the production of mixed magmas, comes the fusion of the eclogite and its ascent again towards the sial. The sunken eclogite is now heated internally and also from above and below by the enclosing peridotite; and sooner or later, according to the subsidiary interactions. a considerable proportion of it must become fused at one time. The combined effects of differential fusion according to melting point, crystal stoping and crystallization of forsterite from the mixed magmas will ultimately lead to the restoration of the peridotite to the depths and the reconcentration of eclogite or its magma to the region immediately beneath the sial. The complications necessarily arising in this process are in accordance with the observed complexities of orogenesis during the Tertiary. The main consequence of Stage III, however, is a period of powerful tension due to the simultaneous fusion of nearly all the eclogite (accompanied by more eruptions of plateau-basalt), followed by a shorter period of equally powerful compression due to its consolidation. Here come the early African rifts, followed later by the main Alpine overthrusts,[1] and the corresponding phenomena in other parts of the world. In Western America and Japan the second andesite-rhyolite-basalt sequence occurred, this, like the first, being a consequence of the fusion of dioritic sial at the base of the continents or folded geosynclines.

*Stage IV.*—Most of the peridotite by this time will have sunk into its normal situation, but it will be hot, and losing heat rapidly into the eclogite above. The latter will thus produce a larger amount of magma than during the normal minor cycles of Stage I, and this amount will be still further added to by the fusion of such eclogite as may still remain entrapped in the upper regions of the peridotite zone. The resulting tension and compression should not, however, be on so spectacular a scale as those of Stage II. The growth of the Mediterranean, the main subsidence of the Red Sea, the second important series of Alpine-Himalayan movements (continuing to the present day in some regions), and the third andesite-rhyolite-basalt sequence of Japan and America are referred to Stage IV.

At the present day we are practically at the end of a major cycle, and in the depths solidity prevails on the whole. Locally magma still exists, mainly no doubt within the sial as a result of the cooling of the former magmas that lay below and gave up their heat.[2] The distribution of modern earthquakes in depth shows that changes of bulk are affecting the substratum through a vast thickness. Mr. R. D. Oldham[3] has recently discussed the evidence in a paper of fundamental importance. Of the episeisms ninety per cent arise at depths less than 8 km., and are due to the sudden fracturing or movement of the rocks of the outer crust. Bathyseisms, however, which have been specially studied by Professor H. H. Turner, originate in most cases at about 200 km., with some from round about 100 km., and a few from depths of the order 500 km. The deeper episeisms barely range down to 34 km., and for these a magmatic origin has been claimed by Japanese seismologists.[1] From these considerations we can form some idea of the minimum thickness of the zone which is concerned in controlling the surface history of the earth.

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[1] See L. W. Collet: *Geog. Journ.*, April, 1926, plate facing p. 304, for diagrammatic representation of the time relations.

[2] See A. L. Day: "Some Causes of Volcanic Activity," *Journn. Franklin Inst.* Aug., 1925, p. 161.

[3] *Q.J.G.S.*, vol. lxxxii, 1926, p. 67.

In conclusion one may hope that the theory of magmatic cycles has been placed geologically and physically on a more secure basis than before. It would be too much, however, to expect that one can have avoided all the pitfalls that beset the wanderer in these unfamiliar fields. Nevertheless, the theory has so far exerted such a remarkable capacity for self-correction, that one may anticipate with some justification that from the network of trial paths followed by the first explorers[2] a well-beaten road will presently emerge by which we can travel still further towards the land of understanding.

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[1] See T. Ogawa: *Jap. Journ. Geol. Geog. Trans.*, vol. iii, 1924, p. 5.

[2] Since this paper was completed a synopsis of an address by Dr. G. W. Tyrrell has appeared (GEOL. MAG., June 1926, p. 284). In this address the application of the theory to petrogenesis as illustrated by the rocks of Scotland and Scandinavia is happily attempted.

# REVIEWS.

A Study of the Oceans. By Professor James Johnstone. pp. viii + 215, with 43 text-figures. London: Edward Arnold and Co., 1926.

THIS book seems to be based on a course of semi-popular lectures and it retains the characters appropriate to such a course. It is easy and interesting to read; but full discussion is not possible and the author sometimes reaches definite conclusions where the evidence is quite insufficient. The reader will be led to believe, for example, that there *was* a Pacific continent, not that there may have been one. The author's aim has clearly been to awaken interest in his subject rather than to deal with it in a rigidly scientific fashion, and for the purpose in view the book seems admirably adapted. One of the special features that distinguishes it from other works on oceanography is the large amount of space devoted to the history of discovery. More than half the volume is definitely allotted to this aspect of the subject, and the idea of the gradual evolution of our knowledge of the oceans may be said to permeate the whole. Paper, type and binding are good, but many of the illustrations are too roughly drawn to be attractive.

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Tinfields of the World. By W. R. Jones, D.Sc., D.I.C., F.G.S., M.Inst.M.M. pp. xii + 423, with 83 text-figures. Mining Publications, Ltd., London, 1925. Price 30*s*.

IT is now twenty years since the appearance of a general work on the tin industry of the world, and in that time many changes have taken place. Some of the older tinfields have approached to, if they have not actually attained, exhaustion, whereas others of great importance have developed. It is obvious therefore that there was a need for such a book as the present, and it is fortunate that it has been produced by an author so well qualified by training and experience for the task. Few men can be acquainted with a larger proportion of the world's important tinfields than Dr. Jones, and where he has not personal knowledge he is well able to co-ordinate and condense the information available, both on the scientific and technical sides of the subject. Some of his conclusions as to certain points of detail in Malaya are even yet matters of controversy, but into this subject we will not here enter.

From the geological standpoint the book is most interesting and instructive. The author's views as to the origin of tin ores, their distribution and relation to other metalliferous deposits and to petrographical types, are thoroughly sound and sane. He accepts fully the latest views maintained by many writers both in England and America as to the genetic connexion between tin ores and granites, especially tourmaline-granites, thus emphasising the pneumatolytic character of the deposits, and laying stress on the zonary arrangement, in accordance with temperature relations, that has been so clearly established in many areas, and in fact is so well seen in Cornwall. Here it is in point of fact the dominating factor in the schemes of development now in progress, which promise a renewed lease of life for this historic mining region.

There can be no possible doubt that the days of alluvial tin-mining are numbered Doubtless new deposits still remain to be discovered somewhere, but the future of the industry at no distant date must depend mainly on lode-mining. The Bolivian industry is here already on a sound footing and Cornwall must, undoubtedly, increase again in relative importance as time goes on. Dr. Jones has wisely devoted a considerable amount of space to a careful and detailed description of the Cornish lode-mines, which may without hesitation be accepted as the typical example, as they are the best-known, of their class, and the careful study that they are now undergoing must eventually lead to important developments in other areas where the production is now mainly or entirely alluvial.

For the general plan of the book we have nothing but praise: the information is clear and to the point, but in one respect we must allow ourselves a criticism, namely, that the author repeats himself a good deal. Many statements of fact, and even whole sentences and paragraphs occur twice or even three times, and we venture to suggest that in the next edition a little revision and condensation could be effected with advantage. The book is admirably produced, recalling strongly some of the best of the American publications on economic geology, and it is commendably free from printers' errors. We have found only a few lapses in spelling in the bibliography, in some of the titles of books and papers in the minor European languages.

This book undoubtedly supplies a need, and is likely to remain for long the standard work on its subject.

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MEMOIRS OF THE GEOLOGICAL SURVEY, SCOTLAND. ECONOMIC GEOLOGY OF THE AYRSHIRE COALFIELDS. Area 1: Kilbirnie, Dalry, and Kilmaurs. By J. E. RICHEY, M.C., B.A., G. V. WILSON, B.Sc., and E. M. ANDERSON, M.A., B.Sc., with contributions by C. H. DINHAM, B.A. pp. 91. 1925. Price 2*s*. 6*d*. net. Area 2: Kilmarnock Basin, including Stevenston, Kilwinning, and Irvine. By E. M. ANDERSON, M.A., B.Sc., with contributions by G. V. WILSON, B.Sc. pp. 107. 1925. Price 3*s*. net.

THESE memoirs are the first fruits of the revision of the Ayrshire coalfields by the Geological Survey, now in progress. The original geological survey dates from 1863 to 1869, and was executed by Sir A. Geikie, Professor J. Geikie, Dr. B. N. Peach, and R. L. Jack. With the more minute mapping and closer investigation, and the extension of mining making much more detailed information available, an immense amount of new material has been accumulated. To accelerate publication of this information a series of short memoirs on special areas, and concentrating on the economic side of the subject, has been planned in advance of the sheet memoirs.

According to the index maps published in each of these memoirs Areas 1 and 2 overlap to the extent of three quarter-sheets of the 6 in. map. This appears to be due to the necessity of preserving some sort of continuity in the account of the bauxitic fireclay of the Millstone Grit, the commercially valuable outcrop of which occurs mainly in the three overlapping sheets. The northern edge of the North Ayrshire basin of Coal Measures is also common to the two areas.

The geology of the Ayrshire coalfield region is much complicated by faulting and by igneous intrusion. During the revision many important new stratigraphical facts have been discovered, which will no doubt be discussed at length in the sheet memoirs, but which, in the works under review, are subordinated to the economic interest. The overlap of the Upper Calciferous Sandstone sediments on to the underlying lavas, both east and west from a central area near Saltcoats, is shown in an interesting diagrammatic section (Mem. Area 1, Fig. 2).

Area 1 is mainly confined to the outcrops of the Calciferous Sandstone lavas, the Carboniferous Limestone Series, and the Millstone Grit; and the chief economic products are the coals and ironstones of the Limestone Coal group (the latter now replaced by imported ore, and the former largely worked out), the bauxitic fireclay of the Millstone Grit, a valuable material the utility of which was discovered during the war, and the coals of the Coal Measures. Only a small part of the North Ayrshire basin of Coal Measures is, however, included in Area 1, and its description is repeated in the Area 2 Memoir, which includes practically the whole of the worked Coal Measures of the Kilmarnock basin.

An interesting point in connexion with this coalfield is its probable extension under the Firth of Clyde. The conclusion is reached that the undersea reserves of coal, although they cannot be estimated, may be very considerable. This favourable view may be taken in conjunction with the recently expressed opinion of Professor R. W. Dron, that a buried coalfield may be hidden beneath the waters of the Firth near Ailsa Craig. Beside the prodominant coals and fireclays, road-metals, limes, and building stones are also dealt with. Both memoirs are admirably written and illustrated, and combine to give us a long overdue account of the northern part of a comparatively neglected field in Scottish geology. It may be remarked that the title on the cover of the Area 1 Memoir does not agree with that given on the title-page, which is to be found at the head of this review.

G. W. T.

GEOLOGICAL SURVEY, CANADA: INDEX TO PALAEONTOLOGY. (Geological Publications, 1847–1916.) Compiled by FRANK NICOLAS. pp. x + 384. Ottowa, 1925.

SCATTERED through the numerous publications of the Geological Survey of Canada are descriptions of fossils, records of their occurrence, and other references. In compiling an index to all these names, Mr. Nicolas has performed a service of value to palaeontologists and stratigraphers. Each entry cites the locality, the figure, and description if there be such, and any notes of interest such as horizon, measurements, special structures, and classification. Species are indexed under the generic name believed to be correct, and cross-references are given from the synonyms. In settling these points, the assistance of the palaeontologists of the Survey, especially Miss A. E. Wilson, is acknowledged by the author. There is also a separate list of trivial names, giving the various genera with which each has been connected. Thus the epithet *canadensis* has been attached to 117 names, of which thirty-two are here regarded as synonyms. Finally there is a " list of fossils whose specific and generic names have both been changed ".

The printing is clear, and the work seems to have been done with care. So far as it has been checked, only one error has been detected: *Arthroacantha* is spelled *Anthroacantha* on each occasion, and is therefore misplaced; the widely used synonym *Hystricrinus* is not cited: presumably it does not occur in the publications of the Canadian Survey. An omission is the name *Turrilepas* from the list under *ottawaensis*. Such slight slips do not detract from our grateful appreciation of Mr. Nicolas' labour.

F. A. BATHER.

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THE NEW ENGLAND-ACADIAN SHORELINE. By DOUGLAS JOHNSON. pp. xx + 608, with 273 illustrations. New York: John Wiley and Sons. London: Chapman and Hall, 1925. Price 42s. 6d. net.

IN his earlier work, *Shore Processes and Shoreline Development*, Professor Johnson has discussed at length and with admirable clearness the general theory of the evolution of coast-lines, and in the present volume he applies to a particular region the principles which he then expounded. The region selected stretches from the St. Lawrence to New York. As he points out, the coastal characters are very different from those of the Coastal Plain south of New York, which he proposes to discuss in a subsequent volume.

The area is formed chiefly of rocks ranging in age from the Pre-Cambrian to the Trias. The folds which have affected them run in general from S.W. to N.E., and accordingly there are belts of rock of greater and of less resistance trending in the same direction. For the purposes of the book these rocks are divided into three groups, resistant rocks forming the uplands, weak rocks forming the low-lands, and unconsolidated deposits. The first group includes the Pre-Cambrian and older Palaeozoic, which have been strongly folded; the second includes the less strongly folded Carboniferous and the very slightly folded Trias; and the last group consists chiefly of glacial deposits. The initial shorelines, the erosional forms, the wave-built forms and the submarine forms associated with each group are separately considered.

The author's general conclusion is that the east coast of North America has suffered differential movement giving a submergence which increases towards the north-east and reaches at least twelve hundred feet in Maine. There have been minor oscillations, producing both submergence and emergence, the last of them being a submergence which culminated only a few thousand years ago. Since then there has been no appreciable change of relative level; but the chief evidence for this last statement is to be found in the Coastal Plain rather than in the Acadian and New England area.

Amongst the special points discussed in the course of the investigation is the origin of the Banks which lie off the coast of Maine. They have often been supposed to consist of ice-borne deposits. Maury suggested that the material was brought by icebergs; Thoulet thought it was carried by shore ice, and several writers have supposed that the Banks are the terminal moraine of the great ice-sheet. From a consideration of the submarine topography Professor Johnson concludes that they are submerged cuestas, the depression which forms the Gulf of Maine representing the lowland which lay on the inner side of the cuestas when the whole was above the sea. His estimate of a submergence of 1,200 feet in this region is based on this idea.

There are many other points of general, as well as of local interest, which are carefully discussed; but it would take us too far even to enumerate them. The book is very readable and the author is eminently fair-minded. He places the evidence fully before his reader and draws his conclusions with greater or less confidence as the evidence seems to warrant them. He generalizes, but his generalizations are not hasty.

Illustrations are numerous and well reproduced. Most of them are maps or sections, but there are also a considerable number of photographic views. It is probably by an oversight that no list of figures is given, but the index to the text is reasonably full. Each chapter is followed by a bibliography, and at the end there is a list of authors mentioned, with references to the text and to these bibliographies.

P. L.

# CORRESPONDENCE.

## VULCANISM AND METALLOGENY.

Sir,—In a paper on Vulcanism and Metallogeny presented to the International Geological Congress which has just been held at Madrid, Mr. W. H. Herdsman refers to my early work on the earth's thermal history, and states that I " found the temperature at the base of the granitic layer and at all greater depths to be about 300° C.". Mr. Herdsman does not, however, go on to say that I immediately concluded that this result was " manifestly absurd " (Geol. Mag., 1915, p. 67). My argument was that if all the heat lost by the earth were made good by radio-activity, and if all the radio-active elements were supposed to be concentrated in a granite layer, then the maximum temperature possible at the base of the radio-active layer would be 300° C. As such a result is hopelessly inconsistent with igneous phenomena, I made the obvious deduction that the radio-active elements were not wholly concentrated in the surface layer. It is perfectly clear in my original paper that I did not commit myself to the " conclusion " for which Mr. Herdsman apparently holds me responsible. What I did was to ascertain the conditions under which a temperature of 1200° C. became possible, and I then adopted those conditions for further discussion in a later paper. There is nothing in either my early or later work which supports Mr. Herdsman's views on ore deposits, and I wish to correct any impression that there may be.

An amusing case of the same kind arose some years ago and is worth recording. In *The Age of the Earth* I gave five different estimates of geological time based on denudation and sedimentation, all of which were of the order 300 million years. I was careful to point out that they were dependent on the assumption of uniformity of rate, and that therefore they could not be supposed to possess any serious value. Nevertheless the figures were seized upon by the American Theosophists as a proof that occult investigations led to sound conclusions. Madame Blavatsky by gazing backwards through time had arrived at a similar estimate of the age of the earth, and had boldly published it in *The Secret Doctrine* at a time when the shorter periods favoured by Kelvin were fashionable. Further progress, however, proved Kelvin to be wrong and Madame Blavatsky right! No reference whatsoever was made to the main thesis of my book, which was that the age of the earth is of the order 1,600 million years.

Arthur Holmes.

University of Durham,
*18th June, 1926.*

## THE EVISHANORAN "ESKER".

Sir,—In Professor Charlesworth's courteous criticism in the Geological Magazine, May, 1926, pp. 223–5, of my paper on the Evishanoran Esker, Tyrone, Geological Magazine, October, 1925, pp. 451–8, he quotes a remark which was made in reference to that esker, but applies it to the Davagh and Dunnamore Eskers, which are very different in shape and composition. I regard the course of those two eskers as consistent with their formation by ice from the east, but the evidence is less definite, and the remark quoted referred only to the Evishanoran Esker with its well-marked bulge to the west. Its course is better shown on the map on p. 453 than on the smaller scale map, p. 457, to which Professor Charlesworth refers. I am glad to find that he had already recognized that the characteristic material of the Evishanoran Esker came from the south-east. I did not refer to what Professor Charlesworth claims as his strongest point, for I fully accept the western origin of the ice during the major glaciation of the locality, and preferred not to discuss the general problems, as after a revisit to the district I still feel unable to accept some of Professor Charlesworth's identifications and the conclusions based on them. Some of the gaps that he attributes to post-glacial overflows I regard as pre-glacial owing to the glaciated surfaces just below, and some of his moraines as drumlins of boulder clay, and others as ridges of aqueous gravel. His preference for the term moraine for the Evishanoran Esker supports my main contention that that formation was deposited on the margin of a glacier and not by a sub-glacial river. From which side the ice came is relatively a detail; but Professor Charlesworth's criticism leaves me still convinced that the Evishanoran Esker was formed beside a sheet of ice on the north-western slope of Evishanoran Mountain after the recession of the main ice sheet.

J. W. Gregory.

*15th June, 1926.*